ECONOMICS AND OPERATIONS RESEARCH: A SYMPOSIUM

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I. OPERATIONS RESEARCH AND ECONOMICS
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OPERATIONS research is a new field so that an assessment is not easily obtained solely by reference to published materials. It is also a highly technical field. A serious survey effort, carefully conducted, must therefore be a time-consuming task and one which would risk frustration by barriers of military and industrial secrecy. Finally, operations research is rapidly changing, so that even a survey combined with interviews of current practitioners would not necessarily yield significant conclusions. Because of demands for their talents, the current crop of practitioners has had to be recruited in make-shift fashion from a variety of fields. Only recently has there begun to appear a small supply of persons trained for operations research as such (mainly at a few engineering schools) and even the curriculum for such training is far from a settled matter.

All of these factors lend themselves to the kind of approach used in this symposium — which attempts to provide some flavor of operations research activities by means of general comments and specific illustrations. From direct experience in the areas to which he addresses himself, each of the authors has attempted to point up shortcomings and possibilities in present and potential relations between economics and operations research.

Professional Requirements

It will be quickly noticed, even in a cursory survey of the literature, that operations research shares its problems with many fields. They are management problems and are therefore *ipso facto* shared with business managers as well as the variety of specialists — in marketing, finance, procurement, logistics, etc. — trained for that field. But there is a striking difference in the tools (and hence viewpoint) which the operations research practitioner brings to these problems. Frequent recourse to instruments of a mathematical or statistical variety forms a part of the normal equipment of the operations research analyst, and he is expected to demonstrate a degree of skill in applying these tools which is not generally required of others who also confront these same (management) problems. In short, the practitioner of operations research is expected to be able to use these tools, standard to most sciences, in a creative and flexible manner, combining them with other considerations, and, if necessary, to exercise ingenuity and courage in going beyond the limits that existing scientific knowledge can validate (at any moment of time). Alternatively, adequate discharge of his responsibilities requires the operations research man to know, at least in a general way, the limits of science, as well as what science has

* This paper is prefatory to those which follow in this symposium. The papers by Baumol and Shubik, cast in the framework of industrial operations research, were originally designed for the Conference of Pennsylvania Economists, Bucknell University, June 13, 1957.

Dr. Enke's paper, though not formally a part of the symposium, serves to complement and extend the discussion. It also provides a concrete example of the process (already commencing) of potential "feedback" between economics and operations research.

to offer in the solution of any particular problem. He may have to draw on scientists for expert and specialized help. It is his responsibility, however, to recognize when this help is needed and also to interpret, guide, and otherwise adapt it for the problems he confronts.

It is useful to think in terms of an ideal. The case can then be likened to that of the medical practitioner. At a professional level, the latter cannot be absolved of responsibility for treating a patient or guiding the measures to counter an epidemic or improve the public health. Neither can he be absolved of responsibility for knowing when and how to use the knowledge of the chemist, the pathologist, the geneticist, and the zoologist. These same characterizations apply to the practitioner of operations research in each area of science and each area of problems on which his practice impinges.

For purposes of concrete illustration, the operations research practitioner may be viewed as an engineer. More specifically, he is a "management engineer." The fact that the term engineer has been (perhaps too strongly) associated with technological applications of science or that the term management engineer has undesirably narrow connotations (arising from its past associations with time and method study) may make it desirable to introduce a new name — viz., operations research.[1]

The term engineer is here used to mean applied scientist. It is meant to characterize a person trained in basic scientific disciplines in a way which equips him with the ability (a) to keep abreast of those aspects of scientific progress which are likely to be relevant to problems he may confront and (b) to draw upon, adapt, and apply this knowledge as each case may require. It also implies a client orientation. That is, it implies a willingness (and readiness) to assist others who are not adequately equipped to use the knowledge and methodology of science in evaluating or solving the problems they confront.

The fact that the management engineer is required to operate upon and improve the forms of management does not itself create problems which have not heretofore been encountered. Consultants and occupants of staff positions provide examples of the channels, long since established, through which the operations research practitioner may move. One may, perhaps hopefully, expect that the grounding in basic scientific disciplines which is presently associated with operations research will help to accelerate the growth of professionalism in these areas. This is needed. Otherwise the exigencies, opportunities, or even mere idiosyncrasies, of practicing management will tend to attenuate the potential contribution which operations research can make to the health and efficiency of organization operations, at any moment of time, and will most certainly impair the full growth of knowledge and methodology in operations research over more extended periods.

Operations research is a highly technical field. In matters affecting its growth or application, management opinion or judgment can generally be accorded lay standing only. This does not mean, of course, that such judgments or opinions should be ignored or that management must assume a subordinate role to the practitioner in dealing with his problems. The analogy with medical practice may be drawn upon again. No doctor can safely ignore a patient's characterization of his symptoms. Moreover, the decision on whether to undergo treatment (preventive or curative) resides, in general, with the patient. The doctor may tailor his treatment and the way he couches his diagnoses to suit the fancy of particular patients. Indeed this often forms a part of the technical requirements of medical practice. But the doctor cannot safely allow the patient's self-diagnosis or prescription to over-ride his own professional judgment any more than he can plead ignorance to current states of scientific knowledge.

Practice and Training in Operations Research

As the above remarks are intended to suggest, the specialist in operations research will require relatively heavy training in the tools of modern scientific analysis. To some persons the reliance of operations research practitioners on "high-powered" analytical devices appears

[1] The use of an appropriately distinguishing name is a problem of statesmanship and strategy which may properly be left to leaders of the profession. The above characterization is for convenience only and not for the purpose of criticizing those who object to equating operations research and engineering (as traditionally understood).

to be only an artifice or an attempt at obfuscation. I think rather that it represents a response to two sets of circumstances which are relevant to the papers which follow. On the one hand, the increasing complexity of management problems has made it necessary to turn to increasingly powerful methods for coping with them. On the other hand, there has been an evolution in the methodology of science which has made it increasingly possible to escape from the confines of the one-variable-at-a-time approach of the classical laboratory. It appears natural that, at some point, these two sets of circumstances should be joined and that some type of person would evolve to effect this joining.

Until recently the preponderance of operations research personnel appears to have been drawn from natural scientists. This is, I think, an historical accident. It appears to have stemmed from two sets of circumstances: as Dr. Hitch notes below, the first appearance of operations research on a significant and sustained scale was in connection with military activities. The problems, at least as initially phrased, appeared to have a high technological content, and the personnel at hand — at least those whom military managers tended to associate with "science" — were drawn largely from the natural sciences. This provided one major set of circumstances. The other set was provided by the social scientists. By and large they were poorly equipped in mathematics and related modes of thought; they were divorced from technology; and they possessed only limited traditions of professional work in the service of management. In view of these deficiencies, it would have been too much to expect them to assume preponderant roles in these early stages of development.

The emphasis on management engineering as one mode of interpreting operations research helps to point up issues which might otherwise be obscured by the course which operations research has followed to date. The examples cited by Baumol and Shubik dealing with inventory, marketing strategy, etc., are typical. They represent what Dr. Hitch calls suboptimization problems, often reconcilable with higher level criteria. They also lie in that area of management which is concerned with what can be called "functional" as distinct from "organiza-

tional" decisions. But management engineering, at least in the sense used here, must ultimately be concerned with designs for management systems and forms of organization as well as with analysis and prescription of decisions within that framework. And, in fact, concern with research and resolution of such problems has begun to be manifest in the literature of operations research. Where important social systems (such as the forms of managed organizations) are involved, it seems clear that the social sciences should supply source material for methodology and insight.

In making these remarks I do not wish to deemphasize the technological content required for adequate training in operations research. Automation and related developments are too close to operations research to permit this. In fact, many business firms (and the military) are beginning to recognize that developments such as electronic computer installations are perhaps best viewed, for business purposes, as part of an integrated operations research approach to their problems. Neither the technological nor computer specialist, nor the person trained only in management, appears to be completely satisfactory for the task of effecting the required adjustments between these two areas and, simultaneously, of providing guidance for future developments in each. It seems natural, therefore, to turn to the operations research analyst, experienced in both areas and able to work with specialists in each, to effect the desired wedding.

I doubt that training in either the social or natural sciences is, alone, sufficient to provide an adequate basis for the practice of operations research as it appears to be developing. Both are required, along with some means of introducing the would-be practitioner to the problems and terminology of management.

This is a tall order and some rather lengthy period of experimentation with educational curricula will be required. The seemingly impossible task of providing a ubiquitously well-trained person in all relevant fields of the social and natural sciences will, of course, need to be alleviated in a variety of ways. Each of the following developments, already in view, and others as well, will provide some assistance. (1) The growth of professional societies, such as

the Operations Research Society of America, should provide guidance as to the nature of the problems and standards of adequate training. (2) The parallel growth of a management science may offer further assistance. The relation of management science to operations research may be likened to that between medical science and medical practice — or it may be likened to that between the scientific (or research) engineer and his applied counterpart. The field of management science, like the field of medical science, will serve as a transmission belt for the practitioner. It will, on the one hand, draw from other scientific disciplines those portions which appear to have "management" promise; on the other hand, it will be devoted to more basic research.[2] Such research will provide the kind of knowledge and improvement in knowledge most needed by the practitioner. It will also stimulate developments in scientific disciplines which are tangential to management. (3) Finally, it may be predicted that, in time, the present "holistic" approach, much praised in some operations research circles, will give way to an increasing specialization in various phases of operations research and so help to ameliorate the training requirements of each candidate.

Some Implications for Economics

I hope that these remarks will help provide some perspective for the discussions which follow. As the papers show, there is need for both breadth and depth of training in economics for operations research. But, as previously remarked, I do not believe that a thoroughly trained economist (at least of the traditional variety) or a thoroughly trained social scientist (if such be possible) will make a qualified (future) practitioner — however valuable such an economist (or social scientist) may be as a supplementary or cooperating resource.

How then is operations research training in economics to be supplied? One road is to incorporate this training in courses devoted to

operations research. Some of this will undoubtedly be done, but I am inclined to believe that much of this training is better administered and taught in professional departments of economics. My reasons are twofold: first, the student is likely in this way to obtain a better "feel" for economics as a discipline and to understand the interconnectedness of economic reasoning; second, it is more likely that some social "leaven" will be supplied, a leaven which might otherwise be omitted or slighted in courses oriented primarily (or solely) to the "useful" parts of economics. Finally, it may be argued that this approach also helps inculcate an attitude in which the student will learn to look toward economics as one of the relevant sciences during his professional career.

The last point is grounded on an assumption that economics is indeed relevant or at least that it can be made so for a person embarking on a career in operations research. Some reorientation of standard economics presentations will be necessary if this is to be accomplished. But this need not imply wholesale revision of course arrangements. Much can be done, for example, by an altered emphasis in the mode of presentation within the standard courses. Because these students will be receiving training in the use of analytical instruments and concepts, it should be possible to use the condensation available from a more theoretical approach in teaching them. It should also be borne in mind that such students are likely to be "problem oriented." This may create difficulties in some instances but it also offers opportunities. Presumably such students are being trained to deal with tools and data simultaneously so that, by proper problem selection, it should be possible to secure reinforcement. In this way some of the danger of superficiality which might otherwise emanate from a broad theoretical treatment may be mitigated. Finally, the suggested inculcation toward economics (if justified) can be used for further reinforcement. In his subsequent career the student may be expected to make sufficient use of the concepts of economics which can supply both motivation and insight. He may do so either "on his own" or as part of an interdisciplinary team of the kind used in operations research work.

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[2] An invidious comparison is not intended and no solid reason can be given for strict compartmentalization between operations research and management science. Management science might be (briefly) characterized as that branch of science which seeks to formulate or adapt generalizations (substantive and methodological) in advance of or because of operations research.

The wide range of economic constructs deemed relevant to operations research is noted in the papers which follow. Even when due allowance is made for the prejudices of economists the range will still be striking.

Another feature which emerges from these papers is the complexity of the problem which the operations research practitioner can expect to confront. As each of the authors notes, economic principles can furnish important guidance in disentangling many of these multivariate or systems mazes. But it must also be remembered that the tools of operations research are often sharp (even humorless) so that the limitations of these guiding economic principles needs to be highlighted in the teaching. Since his problems will often occur in dynamic settings, an operations research practitioner will need to assess the limitations of static ex-ante analysis if he is to use his economic principles with the prudence that his tools require. The same may be said with respect to the "logic of choice" and questions of optimality (including sub-optimization). The assumptions by which an optimally chosen production function is incorporated in the standard economics presentation is a case in point. Such assumptions, unless explicitly revealed, may easily prove a hindrance in work designed to uncover such optima and provide basic analyses from which more rational choices can be made.

Potential benefits to economics are specifically suggested in the papers which follow. I would like to conclude by underscoring the possibilities that operations research offers in the realm of social inventions. Fields such as medicine or engineering furnish examples of what can be accomplished by continued systematic liaison between application and research. Persons who prefer to think of economics unburdened by potential social applications can also view the reverse process by which scientific knowledge and progress has emerged from such collaborations.[3]

[3] A general view of recent endeavors in operations research may be obtained from C. West Churchman, Russell Ackuff, and E. Leonard Arnoff, *Introduction to Operations Research* (New York, 1957); Joseph F. McCloskey and John M. Coppinger, *Operations Research for Management*, Vol. II (Baltimore, 1956); and Joseph F. McCloskey and Florence M. Trefethen, *Operations Research for Management*, Vol. I (Baltimore, 1954). The journals *Management Science*, *Naval Research Logistics Quarterly*, and *Operations Research* provide a means for following developments that are now under way in this rapidly growing field. Automation and its relation to operations research and systems engineering are discussed in Eugene M. Grabbe, ed., *Automation in Business and Industry* (New York, 1957).

II. ECONOMICS AND MILITARY OPERATIONS RESEARCH *
Charles Hitch

What is now usually called "operations research" was first systematically employed in the British and American military services during World War II. The early practitioners were mainly physical scientists — physicists, engineers, mathematicians, and a sprinkling of biologists and chemists. Of course the military services had long used physical scientists in their fields of recognized scientific competence. In operations research groups the scientists turned their attention to strictly military problems of an operational nature.

An example of a typical World War II operations research study reported by Florence Trefethen [1] may make the distinction clear. Some physicists, instead of merely advising on the development of underwater mines, analyzed alternative ways of carrying out mine-laying operations in Japanese waters by B-29 aircraft. Their studies indicated that the B-29's would be most effective against then-existing Japanese defenses if they employed single sortie raids at night at an altitude of 5,000 feet instead of their usual high altitude formation raids by day. When the new tactics were tried our bomber losses fell from about 10 or 15 per cent to roughly one-tenth of that figure with-

* This is a modified version of talks given to faculty and graduate economics seminars at Yale and the Massachusetts Institute of Technology during the spring of 1957 while I was Visiting Research Professor of Economics at Yale. I have borrowed freely from the experience and thinking of many colleagues at The RAND Corporation.

[1] J. F. McCloskey and F. N. Trefethen, *Operations Research for Management*, Vol. I (Baltimore, 1954), 16.

out impairing the effectiveness of the mining operations. Not all World War II operations research was as successful as this, but the instance is not an isolated one.

Since World War II there has been a steady growth in the business of supplying operations research to military and other customers. The old label still sticks, although many of the studies are now concerned not with operations, i.e., with ways of using equipment, but with broader and longer range problems — what military forces it is desirable to have, what kinds and quantities of equipment should be procured, what kinds of weapon systems should be developed.[2] As will become apparent, these broader studies designed to aid military decisions affecting force composition and development programs pose much greater difficulties for operations research than the early operational problems. It is intrinsically harder (for an operations researcher or anyone else) to decide between, say, two bomber development projects which may bear fruit in 1965 or 1970 than to find a good tactic for known bombers to use in penetrating known defenses. Many more variables are involved. Uncertainties are greater. The very basis or criterion for making a decision is harder to determine. "Verification" is tricky and limited at best. Resounding successes, even when they are achieved, are harder to identify.

In this article I will explore some of the relations between operations research and economics. By operations research I mean the use of systematic quantitative analysis to aid in the making of management decisions — in the context of this article, military decisions. This is not a satisfactory definition for several reasons, but perhaps as satisfactory as most accepted definitions of subject matter. I will be mainly concerned with economics as a logic of choice rather than as a study of "those things that can be brought into relation with the measuring rod of money."

Contributions of Operations Research

Before we can evaluate the past or potential usefulness of operations research to the military services, we have to know something about the manner in which military decisions are made. This is not easy to learn, because the military approach to decision making has always been a highly pragmatic one, and military decision makers are not particularly articulate in rationalizing the basis for their choices.

An economist naturally analyzes military problems in terms of his accustomed conceptual framework, using the logic of choice with which we are so familiar. Military problems can be formulated as economic problems of maximization subject to constraints. The nation turns over to the military services certain resources. Some of these are specialized resources — bases, military capital equipment, and trained manpower inherited from past periods. Others are quite general — like the budget, which can with certain limitations be spent on anything, and the raw manpower yielded by the draft. The task of the services is to use these resources in such a way as to maximize our military power or, more ambitiously, "military worth" — the value of those national objectives which are a function of military power. Of course there is no way of measuring military power or military worth — in the general case — any more than there is a way of measuring an individual's, a family's, or a firm's utility or preference function. But the conceptual framework helps greatly even if we can't fill all the empty boxes. It tells us how to seek solutions.

Military and other government officials possess no such conceptual framework. Lacking it, they have developed what might be described as the "requirements" approach. Staff officers inspect a problem — e.g., one pertaining to tactics, strategy, forces, type of equipment, or selection of development projects — and set up "required" tasks or performance characteristics. NATO, as is well known, requires 30 divisions on its central front. The Air Force required 137 wings until budget constraints made that number infeasible. Our next strategic bomber must be developed to satisfy myriad required performance characteristics — a range of X thousand miles, a speed of Mach Y, etc.

How are these requirements determined?

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[2] The broader, longer range studies are frequently called systems analyses. See Charles Hitch, "An Appreciation of Systems Analysis," *Operations Research*, November 1955, 466–81.

Why 30 divisions instead of 15 or 60? Why Mach Y instead of Mach Z? It is easier to say how they are not determined than how they are. An integral aspect of the requirements approach is that cost considerations are excluded until after the requirement has been established — by "military judgment" "on the basis of needs." Then costs are computed in terms of budget, manpower, specific resources, or whatever may be thought to be binding — but only for the purpose of testing *feasibility*. If the requirement turns out to be feasible, it is adopted; if infeasible, it may have to be modified in the direction of feasibility, or Congress may have to be asked for a supplemental budget. Congressmen have frequently demanded that the services prepare their requirements on the basis of needs alone — what we really need for national security, uninfluenced by the sordid economy-mongering of the Treasury and the Budget Bureau. This is, of course, nonsense. There is nothing absolute about national security, especially in this thermonuclear era. Some notion of cost, however imprecise, is implicit in the recognition of any limitation. But all too often this notion involves cost only as a limit or feasibility test, not as a guide to efficient choice among alternatives and use of resources.

There is therefore not merely a vacuum here, a lack of a rationale for choice, but frequently a denial of the relevance of the economist's rationale. For the logic of choice demands that alternatives be costed, in some appropriate sense, prior to choice. It tells us that the choices that maximize military power with given resources are the same choices that minimize the resource cost of attaining that level of power. Unless alternatives are costed (however approximately) *before* requirements are determined we cannot know which of the alternatives will contribute most efficiently to military power.

Military staff studies purporting to recommend or defend decisions give no satisfactory general explanation of their rationale of choice. There is lip service to the "principles of war," which are alleged to be unchanging from generation to generation. These are aphorisms or slogans like "concentration of force" which, precisely because of their generality, are rarely helpful in solving substantive problems. Concentrate forces? Perhaps, but to what extent in what circumstances? How helpful is it to tell a general facing an enemy armed with hydrogen bombs to concentrate his forces? The principles of war bear about the same relation to actual military decisions as Aesop's fables to daily behavior.

So what does lie behind military judgment? Frequently, first-rate intellectual capacity. Frequently, much experience with previous decisions, where hard choices had to be made in the face of resource constraints. Precedents and analogy are almost always important, sometimes for good, occasionally for ill. Individual preferences and prejudices are also inevitably important. The Army alleges that there are Air Force officers who regard with favor the plane that flies fastest and highest — even if its function is to observe camouflaged artillery posts on the ground. There are some officers in all services who look with contempt upon "defensive" arrangements, however essential some may be for preserving our offensive capability. Of course factors of this kind will always influence decisions. But they might influence them less if they had the field less completely to themselves.[3] That is why I regard with so much favor and hope the increasing intrusion into military staff studies of systematic quantitative analysis, even when the operations research is bad or misleading, as it frequently is. I think we can make operations research better. I do not know how to go about making the traditional staff study better except through operations research (or something like it by another name). Precedents and prejudices are not vulnerable to frontal attack.

What has operations research contributed to military planning and decision making apart from some ingenious solutions to isolated problems? I think it has made two tremendously

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[3] I am not suggesting that the "military mind" is any different from any other mind, or any less capable of using reason to adjust to new situations. The myth that the military mind always prepares for the last war could not survive a cursory examination of U.S. military forces today. Precedents and prejudices are pervasively important in business as well as in military life; but the businessman is in a somewhat better position to overcome them because the market supplies him with a dominant and relatively unambiguous criterion or rule of action, and relatively frequent objective tests of his effectiveness.

important contributions of lasting significance:

(1) First, it has had a favorable influence on attitudes. Operations research has persuaded a lot of people, including a good many military planners and commanders, that it is appropriate and useful to approach military problems in a "scientific" spirit of inquiry; that the "principles of war" are not immutable and sacrosanct, but relative, conditional upon circumstances, and subject to analysis; that even systematic quantitative analysis by civilian scientists can on occasion be helpful in making "strictly" military decisions. I do not want to imply that all military planners and commanders have been converted, but most have by now been influenced — even the minority who react negatively and defensively.

(2) Secondly, operations research has demonstrated the tremendous range of alternatives open to those who make military decisions — what economists call opportunities for substitution. There isn't just one way — a traditional way — to achieve some given military objective, but usually an infinite variety of ways, each with its particular advantages and disadvantages, benefits and costs, which must be weighed. Even where the operations researcher has bungled the weighing by choosing an inappropriate test or criterion for determining the best alternative, the arraying of the alternatives has had a most wholesome effect in broadening the vision of military planners.

I do not mean to imply that the operations researchers, or anyone else, have demonstrated the full range of alternatives. Unfortunately, too many operations researchers content themselves with applying some mechanistic test to alternatives suggested by others. But we are constantly pushing the boundaries of interesting alternatives outward. In fact the *invention* of new alternatives, new weapons systems, new ways to accomplish military objectives, may prove to be the operations researcher's most constructive and valuable role. I have observed that economists tend to be less restrained in conceiving some kinds of radically different alternatives than physical scientists. The games we play with hypothetical production isoquants predispose us to emphasize, if not exaggerate, substitution possibilities. Any operations researcher will think of substituting fewer big

bombs for more little bombs. An economist, however, tends to think naturally of bombs, bombers, crews, concrete runways, foreign assistance, and everything else as coming out of a generalized pool of productive resources. Many transformations are in fact feasible in periods that are short compared with the time it takes to develop modern weapon systems.

The diversity and multiplicity of ways in which military objectives can be achieved is central to much that I have to say. It is certainly not generally perceived that the range of choice is so wide. Consider a few examples at different levels of decision making:

a. Suppose that the problem is one of high policy — the provision of some measure of protection to the United States economy against nuclear attack. Broad alternatives include things that do not look alike at all or require the same types of resources: (1) active interceptor and missile defenses; (2) widespread dispersal of industry and population before the attack; (3) shelters and underground construction; (4) full reliance on a striking force as a deterrent or (in some circumstances) to destroy the enemy striking force on the ground. These are only the broad, pure alternatives; there are all the permutations as well as any number of specific ways of accomplishing each of the four.

b. Or consider a problem at a somewhat lower level. Suppose we have decided that despite missile development we want an atomic striking force of manned bombers during, say, the 1960's, but somehow have to extend the range of high performance aircraft so that bombs can be dropped in the heart of Asia. How can we do it? Broad alternatives include (1) operating from bases far forward on land; (2) operating from bases far forward afloat; (3) staging bases forward (on land or sea) to refuel on the surface bombers operating from the United States; (4) extensive air refueling operations with bombers operating from United States bases; (5) larger aircraft with greater fuel capacity. If there is time enough for a development program, many other alternatives appear — the development of engines with lower specific fuel consumption; the development of higher energy fuels; the development and adaptation of lighter weight structural ma-

terials, like beryllium; and so on. All these are alternative ways to buy range in aircraft.

c. At a still lower level, consider the problem of designing a new machine gun. There are "trade-offs" or exchange rates among various *performance* characteristics (range, accuracy, lethality of bullet, durability, reliability — some of which may have high military worth, others a merely traditional or perfectionist kind of appeal); and between each of these and *physical* characteristics (weight of gun, weight of ammunition — which may grossly affect the size or effectiveness of the unit using the gun). There are trade-offs too between each performance and physical characteristic and the costs both in money and in the time it will take to develop the machine gun and get it into operational use.

Each of these problems is an economic problem of efficient use of resources — a problem of maximization under constraints. Each requires for its solution a definition of objectives, a systematic analysis of each of the alternative ways of achieving the objectives, using appropriate models of reality, and the selection of a preferred alternative on the basis of some good test or criterion that relates the costs or resources required by each alternative with its effectiveness in achieving the objectives.

It should come as no surprise to economists familiar with international and inter-firm differences in productivity that some of these alternatives turn out to be much more efficient than others. It is not unusual for operations researchers to find that one means is several times as efficient in accomplishing an appropriate quantitative objective as other means that appear equally plausible and have equally enthusiastic supporters. In fact, Morse and Kimball in their classic work advise the operations researcher not to bother unless he can find differences of a factor of three or more.[4]

The Criterion Problem

The magnitude of defense expenditures is now so great, and the possible consequences of

[4] P. M. Morse and G. E. Kimball, *Methods of Operations Research* (New York, 1951), 38. I am citing this reference as an independent estimate of the range of opportunities in some military problems, not because I agree that factors of improvement less than three should always be ignored in operations research.

failure in defense so catastrophic, that we need good methods to choose some of the more efficient alternatives at the various levels and reject less efficient ones. In the private economy we have a price mechanism and a system of incentives which, very imperfectly but pervasively and persistently, promote the selection and survival of relatively efficient methods. In the government, including the military, there is no comparable system. There is no surer way to appreciate the beauties and merits of the price system than to spend some time contemplating the chaos where it doesn't exist and nothing replaces it — where what correspond to production and purchasing decisions are made on grounds that are adventitious or just plain wrong.

Operations researchers have begun to introduce some rational calculus into this chaos. They scored some striking if easy successes during the war, and have made some progress in dealing with more complex problems since the war. They have discovered and demonstrated that the traditional, conventional, or plausible way of carrying out a military task is not the only way; that there are almost always many other ways, some of them much more efficient than others. They have made considerable progress in developing appropriate techniques such as operational gaming and models such as linear programming for dealing with complex special problems. What they have not done is to deal professionally, constructively, or even thoughtfully with one of their own most fundamental problems — the selection of criteria or tests of preferredness. They presume to advise decision makers, but they lack a theory of choice. Here is where I think economic theory and economists with good economic intuition can be especially helpful.

To a very considerable extent this failure of operations researchers results from lack of trying. A theory of choice implies a theory of value, and physical scientists tend to shy away from value systems. Moreover, nothing in their training suggests, even by analogy, how they might proceed to construct one. If you pose a military problem — say, the defense of the United States against nuclear attack — to a group of physical scientists and to a group of economists, my experience is that the two

groups will set about solving it in strikingly different manners.[5] The physical scientists will start almost immediately with the characteristics of the hardware systems alleged to be available, and with the design of analytic models (e.g., of possible air battles) to reflect and predict the empirical world. The economists, by contrast, will usually begin by asking what we really want to do; what our national objectives are; what broad alternative means there are to achieve them; what test or criterion we can use to select the best or a good one in the light of national objectives. In the end the physical scientists have to find a criterion and the economists have to consider hardware and build or borrow some models, but in neither case is this because they are saving the best till last, so that the taste will linger.

My conviction that the main shortcoming of much current military operations research is its failure to cope with the criterion problem is shared by Bernard Koopman, last year's President of the Operations Research Society of America.[6] How does an operations researcher typically go about choosing criteria — choices to which the results of his analysis are typically far more sensitive than the choice of mathematical models, to which inordinate study and debate are usually devoted? In some cases he casually takes the first criterion that pops into his mind and dashes on to the less important but more congenial aspects of his job. In some other cases he falls back on one of what Professor Koopman calls operations research's "procedural fallacies," either:

Authoritis — letting the customer (probably a general or admiral) choose the criterion, even though this involves letting the customer "ask the question" or "define the problem," a responsibility no self-respecting scientist would abdicate to his customer, for good reasons,

when he is on his own familiar ground as scientist; or

Mechanitis — putting his machines to work as a substitute for hard thinking. Because he lacks any rationale for choosing a good criterion, the operations researcher writes down all the objectives he wants to accomplish and all the costs he wants to avoid, links them in various permutations to form criteria and lets the machine optimize for each criterion in turn. Then either he bases his recommendations on some form of majority vote (this might be called the fallacy of misplaced democracy — all criteria are inherently equal) or he combines the fallacy of mechanitis with that of authoritis, passing all results on to the customer who, thoroughly confused by a welter of apparently equally plausible solutions, has to make the choice.

Certainly the criteria that have been used by operations researchers have frequently been inferior or misleading. Moreover the really bad ones involve errors that I think would not have been made by a person with good economic intuition. Let me cite a few typical examples. The first is a famous, skillful, and influential piece of World War II operations research — the convoy problem discussed in Chapter 3 of Morse and Kimball.

The data here revealed that, over a wide range, the number of merchant vessels sunk in a U-boat attack on a convoy was proportional to the number of U-boats in the attacking pack and inversely proportional to the number of destroyer escorts, but independent of the size of the convoy. They also revealed that the number of U-boats sunk per attack was directly proportional both to the number of attacking U-boats and to the number of defending escorts. The criterion was taken (plausibly) to be the maximization of an "exchange rate" or ratio of enemy losses (measured in U-boats) to our losses (measured in merchant ships).

I quote the conclusion: "The important facts to be deduced from this set of equations seem to be: (1) the number of ships lost per attack is independent of the size of the convoy, and (2) the exchange rate seems to be proportional to the square of the number of escort vessels per convoy. This squared effect comes about due to the fact that the number of merchant vessels

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[5] I am *not* suggesting that the approach of either group is "better," in the sense of being more productive. Both approaches are incomplete; each complements the other.

[6] Professor Koopman, a mathematician, refers to the choice of "figures of merit" and "measures of effort." In my terminology the former are objectives, the latter costs, and the criterion usually some relation between them (difference; ratio; maximization or minimization of one with other as constraint, etc.). See Bernard O. Koopman, "Fallacies in Operations Research," and Charles Hitch, "Comments," in *Operations Research*, August 1956.

lost is *reduced*, and at the same time the number of U-boats lost per attack is *increased*, when the escorts are increased, the effect coming in twice in the exchange rate. The effect of pack size cancels out in the exchange rate. From any point of view, therefore, the case for large convoys is a persuasive one.

"When the figures quoted here were presented to the appropriate authorities, action was taken to increase the average size of convoys, thereby also increasing the average number of escort vessels per convoy. As often occurs in cases of this sort, the eventual gain was much *greater* than that predicted by the above reasoning, because by increasing convoy and escort size the exchange rate (U-boats sunk/ M/V sunk) was increased to a point where it became unprofitable for the Germans to attack North Atlantic convoys, and the U-boats went elsewhere. This defeat in the North Atlantic contributed to the turning point in the 'Battle of the Atlantic'." [7]

This happy outcome depended on the good sense, and especially on the intuitive restraint, of the participants rather than upon a sophisticated choice of criterion. The criterion selected can be criticized from many points of view, some of which are discussed below. It is rather typically not explained or defended. There is no evidence that the researchers stopped to ask: what is the purpose of this convoy business anyway; what are we really trying to achieve by it? Well, what were we? Certainly not merely the sinking of U-boats or the saving of merchant ships, let alone one divided by the other! The function of the convoys was to get material across the Atlantic for the supply of England in preparation for the invasion of the Continent. This was the contribution that convoys made to the achievement of our objectives at a higher level — prompt and decisive victory over Germany.

The criterion chosen could have been consistent with our higher level objectives only by accident and over a limited range. For it completely neglects a dimension of the problem that appears at least as important as those considered — viz., the reduced operating efficiency of ships in large convoys, and hence the *inverse* relation between the size of convoy and

[7] Morse and Kimball, op. cit., ch. 3. Italics in original.

the capacity of any given number of merchant ships to transport men and materiel across the Atlantic. Collecting large convoys takes time. The arrival of large convoys swamps port facilities, which means longer turnaround times. Because the speed of a convoy cannot exceed that of the slowest ship, there will be an inverse average relation between its size and speed. Thus, it is not true that the case for large convoys is persuasive "from any point of view." It might well be worth the loss of a few more ships to insure the delivery in time of the supplies required for the Normandy invasion. The omission of these considerations is the more curious because they seem so admirably adapted to analysis by the quantitative methods of operations research.

That something was wrong with their criterion should have been immediately evident to the operations researchers: it proves far too much. It shows that it would be desirable to increase the size of convoys without limit — until the whole merchant fleet and all the destroyers were assembled in a single convoy. We are warned, it is true, that the equations cannot be expected to be valid for "very small" and "very large" values — but this is a conventional warning against extrapolating functions far beyond the range of the data from which they are derived. The important point is that, long before the whole merchant fleet becomes a single convoy, the significant reductions in losses will have been achieved and the reduction in the efficiency of ship use will have become unacceptable. We will always need judgment and good sense in interpreting and applying the results of operations research, but we must try to find criteria that place a less overwhelming burden upon them. This requires, as a minimum, thoughtfulness in choosing the criteria and a careful consideration of the relevant objectives at the next higher level.

This same example can be used to illustrate another unfortunate tendency of operations researchers in choosing criteria — a preference for *ratios*. The training which leads economists to prefer *differences* is a sound one: it makes sense in any activity to maximize total profits in some sense, not profits per unit. If incommensurability of objectives and costs prevents finding differences, the nearest approxi-

mation is not to divide one by the other; it is to maximize objectives subject to cost constraints or to minimize the costs of attaining defined objectives — criteria that are logically equivalent for any given level of objectives or costs. Ratios are treacherous as operations research criteria because they ignore the absolute magnitudes of both numerator and denominator.

A particularly common and vicious form of ratio criteria is the one that maximizes an objective divided by some single valuable input, when other valuable inputs are required. It is apparently altogether natural for the non-economist to optimize on the scarcest or most valuable resource, treating other inputs as if they were free goods. More frequently than not operations research studies of nuclear bombing systems have maximized destruction per gram of fissile material — completely ignoring far more costly delivery system inputs, and getting weird results. It is seldom that all but one input can be regarded as free, with no alternative uses. An economist would not fall into this trap.

The ignoring of some cost elements may in turn be regarded as a special case of another extremely common criterion error — the neglect of effects, which may be either harmful or beneficial, on other military or national objectives. The typical operations research study dealing with strategic air problems simply ignores important impacts of bomber deployment on air defense, as well as possibilities of jointly supplying some tactical offensive power along with the strategic. Economists are trained to look for external economies and diseconomies, for divergences between private and social products and costs, and for joint products and costs. These are exactly the concepts that are needed, and they have not come naturally to many physical scientists practicing operations research.

I will provide one more example. During World War II an operations research project to compare the effectiveness of different bomber aircraft used as its criterion the ratio of allied-man-years-in-bombing-effort to enemy-man-years-on-defense-and-indispensable-repairs.[8] Let us pass over the indefensible use of the

ratio; the use of man-years, which ignores requirements for special skills and capital in systems which make heavy demands on both; and the inclusion of sunk costs — which seems to arouse righteous horror only in the breast of an economist. Here is a criterion that would score zero for a bombing system so effective that it made the enemy give up as hopeless any defense and all attempts to repair damage. What were we trying to do with our bombing system? What was the allied objective here with which our low level criterion must be consistent? How can operations researchers be made to keep their eye on the ball — the right ball?

Economic Theory for a Non-Market Economy

I have used the inelegant word "sub-optimization"[9] to refer to operations research at low levels in a decision hierarchy — at levels lower than some in which the decision makers are (or ought to be) interested. Sub-optimization is not a derogatory term: I am a confirmed sub-optimizer. The operations researcher, using quantitative methods, must cut his problem down to workable size. He frequently *can* do so and reach valid and useful conclusions by choosing with care criteria that are consistent with objectives at higher levels. It is not always necessary to *solve* global problems in all their complexity in order to recognize their general features — a happy circumstance that makes much useful operations research possible. The operations researchers working on the convoy problem did not have to optimize the whole grand strategy of war against Germany to know that the function of Atlantic convoys was to move supplies across the Atlantic. As economists we know that we don't have to *solve* the global problem of efficient allocation of resources in an economy to determine what kinds of maximizing activities by firms in what circumstances are conducive to such efficient allocation. We can deduce from quite general characteristics of the economy that, for example, in certain circumstances profit maximizing does have this tendency, but that man-hour-productivity maximizing would not — a conclusion by

[8] Charles Kittel, "The Nature and Development of Operations Research," *Science*, February 7, 1947, 152–53.

[9] Charles Hitch, "Sub-optimization in Operations Problems," *Operations Research*, May 1953.

no means obvious to some brilliant people lacking economic training.

Much of micro-economic theory is basically concerned with the relations between criteria at different levels: usually, but not exclusively, between criteria of the firm and the consumer at a low level, and of the economy at a high one. This problem is the special concern of the ill-named and much-defamed "economics of welfare." Welfare economics, despite its narrow and vulnerable philosophic base, constitutes a considerable intellectual achievement. It has developed original and fertile concepts and a method of analysis that can be applied to problems having nothing to do with welfare and consumers' utility. In fact, what operations research seems to me to need if it is to fulfill the hopes of its founders is a sort of generalized welfare economic theory to guide its choice of low-level criteria. The theory needs to be generalized and developed in several directions:

(1) To consider a variety of high-level objectives or criteria, not just those of traditional welfare economics. We want military suboptimizations to contribute to, say, deterrence of war and other definable national objectives rather than to consumers' utility.

(2) To consider varying the low-level criteria or decision rules themselves as an administrative means of control. What function should we ask the Post Office Department to maximize? The postmaster in Cambridge? The Air Defense Command? The unit responsible for maintenance at an Air Defense Command base? Welfare economics typically *assumes* the low-level criteria (profit maximization in the case of firms, utility maximization in the case of consumers) and studies the consequences for high-level objectives of manipulating the market environment (e.g., by tariffs, taxes, subsidies).[10]

(3) To relate criteria at decentralized decision levels with *incentives* and *information* at the same levels. It does little good to ask a subordinate official to maximize a function if

this requires data he cannot get, or if it runs counter to his self-interest. The cost of getting the information to the appropriate officials should be an input of any analysis. Incentives, as in the market economy, can make or break any plan. In some cases the devising of appropriate incentives may be more rewarding than operations research on the substantive problem.[11]

(4) To consider environments in which markets are the exception rather than the rule.

I am not suggesting that any of this is easy or that it can be done quickly, or that I know how to do it, but I am persuaded that economists will find the job more congenial than members of any other discipline.

Briefly, the most important role of economists in military operations research is conceptual rather than substantive. Economists now serve hand-maiden roles in most operations research organizations, supplying project leaders with "required" parameters that are generally considered to be economic, like cost factors or the effects of bombing industrial targets. What more of them ought to be doing is aiding in the design of operations research studies as *economic* studies to achieve efficient use of resources. They can do this by practical participation in substantive work, by theorizing, or preferably by a mixture of both. Here is an all-too-rare opportunity to apply micro-economics to interesting, important problems.

Paradoxically this role for the economist is more important in military and other governmental areas than in industrial operations research. There are difficult and intricate criteria problems in industrial operations research — of which many economists are too little aware. But they are nothing compared with criteria problems in the military. Profit maximization looms predominant in the background of almost all industrial operations research. If the operations researcher shows any persistent tendency to forget it, economists are not needed to remind him — his customer will. The military operations researcher lacks this kind of guidance.

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[10] An interesting exception is the work of A. P. Lerner, Oscar Lange, and others on a rational socialism and on the price/output policies of nationalized industries, which develops criteria or "rules" for state enterprises. But they consider only enterprises selling goods on markets.

[11] On both information and incentives see Tjalling Koopmans, "Allocation of Resources and the Price System," in his *Three Essays on the State of Economic Science* (New York, 1957), esp. 4–23.

In filling this role of conceptualizer, economists have advantages, which I may have overstressed. But we also have disadvantages and shortcomings, including some "trained incapacity" of our own. High among them, I think, are our too highly developed drives for *generality* and *perfection*. We insist on setting our sights unreasonably high. Some of us are "grand optimizers." We want to generalize at least for the whole Department of Defense no matter how unmanageable this makes the analysis; we want to find optimum solutions instead of just better or good ones; we want to be able to prove, with 100 per cent certainty in each particular case, that our solution is optimum. Typically we can do this only for paper problems that we invent.

Witness the reaction of many economists to the "efficient point" concept. What good can the concept be? You cannot prove that achieving efficiency is desirable, because some inefficient points may well be better than some efficient points.

Actually, the efficient point concept, when imaginatively used, is a flexible and powerful tool of operations research. It does not enable one to find the elusive *optimum optimorum* because there may indeed be inefficient (or efficient) points superior to any particular efficient one. But it does enable one to do something of greater practical significance — to travel northeast, to show that solution B is better than solution A, when solution A is the particular inefficient point representing the likely alternative or what is currently planned or programmed. The practically important thing is that you can always find one or more efficient points superior to any particular inefficient point.

Of course we shall not be able to prove that a solution is optimal for the Defense Department or the United States as a whole. But we do not have to. If in the hierarchy of military requirements we can make choices more efficiently at *any* level, we are likely to improve the inefficiency of the *whole system*.[12] Even if a require-

ment at some level is arbitrary and not subject to critical analysis, meeting it efficiently frees resources for other uses. And we do not have to be sure that we are right; there are many decisions, and we can be satisfied with a good probability of being nearly right in any particular case. We can play the percentages. *The acceptance of a philosophy of the "second best" is a prerequisite to any progress in solving substantive problems in non-market economies.*[13]

My experience suggests that economists have as much to learn as to teach by engaging in this kind of systematic quantitative economic analysis. If we have a comparative advantage, vis-à-vis the physical scientist, in dealing with criteria and related conceptual problems, most of us are at a comparative disadvantage (or no good at all) at designing systems to meet the tests of our criteria, or at contriving models of the empirical world to test their behavior. This sort of thing we have left so exclusively to the entrepreneur — by assumption — that we do not really know what his problems are, let alone the substantive problems of economic efficiency in the government.

One thing that economics might hope to gain from an alliance with military or governmental operations research is a new branch of the subject to fill what has become a yawning gap — an economics of government expenditure. Ever since Adam Smith we have been so bemused by the idea of markets that we have almost completely neglected those large sectors of the economy — mainly in and supplying the government — in which markets (and associated prices and incentives) are either lacking or incomplete. Public finance contains few clues to how the government should *spend* money or use resources; its principal concern has been with how to *raise* money.[14] This gap

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[12] This is not necessarily true if decisions are being made concurrently at different places and levels. Economists are at least sufficiently aware of the pitfalls of *ceteris paribus*. But (1) the fact that it is not necessarily true does not prejudice the expected value of operations research unless there is bias; and (2) this is another good reason for

operations researchers to keep themselves aware of problems (and developments) at higher and collateral levels.

[13] For discussions of "second-best," see I. M. D. Little, *A Critique of Welfare Economics* (Oxford, 1950), esp. ch. VII, 110–20, and ch. XV, 267–72; J. E. Meade, *The Theory of International Economic Policy*, Vol. II, *Trade and Welfare* (Oxford, 1955), esp. ch. VII, "The Marginal Conditions for the Second-Best," 102–118; and R. G. Lipsey and K. Lancaster, "The General Theory of Second Best," *Review of Economic Studies* (1956–57), 11–32. Further developments of the theory are urgently needed, as well as efforts to state explicitly the conditions for particular improvements.

[14] This is not to say that economists have been com-

in economics was perhaps not too important before World War II when western countries, except during wartime, spent very small proportions of their resources on defense and not much more on civil government. But it is un-

deniably important now, and with a few exceptions economists are doing nothing, letting the only practitioners in this game advise government departments, in effect, to maximize the ratio of U-boats sunk to merchant vessels sunk.

pletely uninterested in the analysis of public expenditures. Many books on public finance have devoted a few pages to the principles of allocating government outlays, pointing out that expenditures on each output should be such that incremental gain (approximately) equals the incremental cost. Pigou focused attention on these principles in *The Economics of Welfare* (1st ed., London, 1920). Most of the earlier writings pertained to general conceptual issues, though a few economists have examined specific governmental purchases, e.g., H. Simon and C. Ridley in *Measuring Municipal Activities*, International City Managers' Association (1943). In recent years economists have become increasingly interested in public expenditures. For example, see Arthur Smithies, *The Budgetary Process in the United States* (New York, 1955); Jesse Burkhead, *Government Budgeting* (New York, 1956); Paul Samuelson, "The Pure Theory of Public Expenditure," this REVIEW, XXXVI (November, 1954); various volumes on welfare economics; and

forthcoming books by Otto Eckstein (*Water Resource Development: The Economics of Project Evaluation*, Harvard Press), R. N. McKean (*Efficiency in Government through Systems Analysis*, John Wiley & Sons), and R. A. Musgrave (*Theory of Public Finance: A Study of Public Economy*, McGraw-Hill).

Even today, however, most economists look chiefly at (1) the conditions for a broad optimum rather than the necessary and sufficient conditions for improvements, and (2) outputs that have market prices rather than the far more numerous and important outputs of government that are not marketed. Hence, while it is gratifying to find the increased concern with public expenditures, far more attention needs to be given to specific substantive problems of choice, to efficiency in producing non-marketable outputs, and to the possibilities for sub-optimization and "second-best" solutions.

III. MARGINALISM AND THE DEMAND FOR CASH IN LIGHT OF OPERATIONS RESEARCH EXPERIENCE *

William J. Baumol

Operations research work can be useful to the economist in two ways: by permitting him to observe directly the operation of real firms and by providing him with new analytic tools. By letting the economist-operations researcher see how firms really work, the businessman offers him the data with which he can increase the realism and relevance of his models. In addition, methods of operations research provide the economist with new equipment for his analysis. In this paper I shall draw upon personal experience to offer a sample of each of these two ways in which operations research has helped and can help further to enrich economic theory.

First let me illustrate how direct observation of the operation of real business enterprises can cast light on economic theory. It is several

years now since the marginal cost controversy.[1] It will be recalled that the issue was whether businessmen really maximize profits by taking advantage of every reasonably obvious opportunity to make profitable incremental adjustments in their decisions. It was contended by one side that, whether consciously or not, rough marginal adjustments are made in practice. The marginalists argued that few businessmen will knowingly reject legitimate increases in their long-run earnings, and this is about all that marginalism asserts. For inequality between marginal costs and revenues means that the businessman is not taking advantage of an opportunity to increase his profits merely by changing the magnitude of his output. Though the businessman is often untrained in the mar-

[1] For the marginalist position see Fritz Machlup, "Marginal Analysis and Empirical Research," *American Economic Review*, XXXVI (September 1946). Examples of "anti-marginalist" views are R. L. Hall and C. J. Hitch, "Price Theory and Business Behavior," *Oxford Economic Papers*, No. 2 (May 1939); and R. A. Lester, "Shortcomings of Marginal Analysis for Wage-Employment Problems," *American Economic Review*, XXXVI (March 1946).

ginal geometry or the differential calculus, a combination of judgment, experience, and trial and error may permit him to achieve a good approximation to profit maximization. If this is so it is a matter of simple mathematics to prove that, even if they have never heard of the concepts, businessmen must be operating on a scale at which marginal costs are approximately equal to marginal revenues.

The opponents of this view based their position largely on direct interviews with executives. These suggested that many business pricing decisions are made on a so-called full-cost pricing basis. Such prices are computed by adding what is considered a reasonable mark-up to an average (rather than to a marginal) cost figure. Further, the anti-marginalists have suggested that this pricing method is not wholly irrational. It is clear that this procedure has the advantage of simplicity and low computation cost once an acceptable mark-up figure has been determined. It also offers some degree of insurance against loss if the volume of sales on which the average cost estimate is based has been estimated correctly or if average costs do not change very much as the scale of output varies. Of course if, for example, the computed price does drive customers away, and if average costs rise sharply when operations are curtailed, then a "full-cost price" may end up leaving a substantial proportion of costs uncovered.

Finally, there is an argument which maintains that this method of pricing serves the businessman's long-run interests in a more subtle way. Short-run profits which are very high can attract new competitors, or they may arouse the interest of the Federal Trade Commission. Either of these events can, in the long run, cost the firm dearly. The businessman's long-run interests may then best be served by avoiding equally both very low and very high short-run profit rates. The full-cost price, because it aims at a predetermined mark-up, may be well designed to achieve just this goal. Similarly, long-run considerations may argue against a price as low as that called for by short-run marginal analysis, because (in Marshall's well-known phrase) it may spoil the market by activating price competition and reducing the price at which future sales can be made.

It should be noted in passing that if full-cost pricing does in fact best serve the businessman's profit interests then, by definition, the marginal-cost pricing decision will necessarily coincide with that which follows from the full-cost pricing procedure. For a full marginal calculation should (ideally) take account of the clerical costs of computation, of the probability of loss, and of the expected long-run effects of any decision. If it is known that high current profits will in the long run be costly because they induce new competitors to enter the industry, this fact will be taken into account in a correct marginal price-output computation.

The marginal-cost controversy is then an issue which can be settled only by empirical observation. It hinges on the way in which businessmen do actually arrive at their price-sales volume decisions. I myself have little confidence in the evidence obtained from interviews with the decision makers. It is almost impossible to find out whether problems of communication, respondents' integrity, and willingness to supply data have somehow been overcome. But even if these have in some way been taken care of, we must remember what the psychiatrist knows so well — that the last person to ask about the motivations which lie behind a decision may be the man who made it. As the marginalists pointed out, the businessman is not precluded from thinking marginally by the fact that he goes through the form of setting price at unit cost plus a mark-up. For he still must somehow decide on the appropriate mark-up figure, and there is nothing to prevent him from choosing that mark-up figure in a way which as nearly as possible maximizes his profits.

Operations research work is peculiarly well suited to shed light on this matter. The operations research worker is often required to examine carefully previous decisions and the data on which they were based. He can then check over the calculation of the gross margin to see whether it did make for maximum profits insofar as these can be inferred from the available data. In addition, there is a stronger test available to the operations researcher. His own advice is characteristically marginal in that his recommendations are frequently based on profit maximization computations. For our purposes, for example, mathematical programming may

be described somewhat parochially as a new tool of the marginal analysis. The thing to be watched, then, is the businessman's attitude toward the operations researcher's recommendations. In this way we can find out how management reacts to marginal analysis which is handed to it on a platter. The case of the marginalist would be dealt a heavy blow if it were found that the businessman regularly rejects this sort of advice after he has received and paid for it.

Though operations research experience is, therefore, highly pertinent to the marginal-cost controversy, it involves, at least so far, a sample of case studies which is small (though growing rapidly) and which is by no means randomly selected. The results of this experience are, therefore, best reported impressionistically.

It is my own experience that (as was doubtless to be expected) the truth lies somewhere between the two contending views. One does occasionally encounter business decisions in which marginal reasoning was used correctly. Very recently the district manager of a steel company told me that even though it is unloaded in the middle of the day he computes the cost of handling a badly stacked truckload at the overtime pay of his warehousemen because this causes them to work more overtime hours that week. That same manager also indicated that he was pushing a line of stainless even though the standard company computation indicated that a district office made no profit on that particular item. He argued that any *additional* sales he brought in would add little to his costs. The old line, "I'm losing a nickel on every item, but if I sell enough I can break even," then turns out to be a piece of marginalist propaganda!

I have more often seen *major* decisions made on the basis of marginal or, rather, incremental reasoning. For example, a proposal for an enormous advertising campaign was turned down by a large household product manufacturer despite the fact that such a "crash program" is the standard approach of his most successful competitor, because it was thought that even in the unlikely event that the computed maximum "market potential" were achieved the proposed additional advertising cost would not be covered. And I have seen the executives of a

large manufacturer of flour and flour products reason in this way when deciding not to introduce a new product. In major decisions of this sort marginal reasoning is apparently more obvious — it is difficult to avoid seeing that the relevant question is "What will this bring me?"

But in day-to-day operations it is equally clear that marginal reasoning is frequently absent. These decisions are made by rule of thumb not only because such a rule saves decision-making time and effort but more frequently, as the executive ruefully admits, because he knows no better way of going about it. That is often why the operations research man is called in — the executive knows that his rule of thumb is all wrong.

Examples are easily cited. Despite the illustration of the influence of marginal reasoning on advertising cited above, promotional budgets are frequently set at some arbitrary fixed proportion of sales revenue although it must be admitted that executives to whom I have spoken about this do worry about what that expenditure brings in to them. Of course our relative ignorance of just what advertising actually does for the firm makes it difficult to advocate alternative procedures in this area.

Inventory control is perhaps a more illuminating example of the role of rules of thumb. The household products company mentioned above tried to keep its field warehouse inventory at an admittedly arbitrary level equal to three times its weekly sales volume. Similarly the steel company set up a target in terms of turnover though the executives recognized that differences in customer characteristics and products sold in different districts must result in deviations. An analysis of the inventory history of the household product manufacturer showed, as might have been expected, that this rule resulted in rather consistent deviations from a most profitable inventory program. Fast moving items are sold to many different buyers and erratic individual purchase patterns are likely to be swamped. As a result, for such commodities relatively smaller safety stocks can usually do the job. And the computation showed that optimum safety stocks of these large volume items were consistently far below their current inventory levels, which were based roughly on the standard three week target.

Similarly, small volume commodity inventories were almost uniformly too low. It turned out to be possible to reduce total stocks over 40 per cent (since large selling items naturally contributed the bulk of inventory) and yet at the same time to achieve a really impressive reduction in the number of times items were out of stock.[2] Here is a clear-cut case where a rule of thumb did not make for maximum profits, short run or long run.

Similarly, full-cost pricing is often what it is advertised to be — a device which cuts down on profits. This is particularly clear where fixed costs are relatively high. To the uninitiated one of the marginalists' most surprising recommendations is that (subject to some limitations) fixed costs be kept out of the pricing decision because, by definition, these are costs about which nothing can be done. Any price-output combination which maximizes profits in the absence of fixed costs must necessarily maximize them after all truly fixed charges are deducted. But fixed costs increase the average-cost figure and raise the full-cost price. For this reason, in one rather typical case, the manufacturer of a fuel kept his price just slightly above that of the nearest competing fuel. Yet experience in other areas indicated that the price reduction required to make our manufacturer's product "competitive" would not lead to a fall in the price of the rival fuel[3] and could be expected to produce substantial increases in sales and in profits.

But even though many of their decisions apparently run counter to the marginal theory, my experience also suggests that many businessmen, when the matter is carefully explained, are delighted to learn the techniques and to adopt many of the results of a marginal analysis. For example, the fuel manufacturer just discussed was highly satisfied with our analysis and very willingly accepted the recommended price reduction.

In another case a producer of a nationally distributed popular beverage found it was losing sales to less expensive local competing brands. It asked us whether a reduction in the price of its products to competitive levels was advisable. The manufacturer was readily convinced by a marginal type of argument which showed that the price reduction would almost certainly have been unprofitable. Just to make up for the proposed reduction in price it was estimated that demand had to be of elasticity greater than 3, and a 70 per cent increase in sales volume would have been required. This seemed highly unlikely but, more important, the manufacturer did not even possess the capacity for the 70 per cent increase in output necessary to prevent a reduction in profits if the price had been cut.[4]

Similarly, in my experience, marginalist advice on inventory levels is almost always greeted with approbation, indeed, usually with enthusiasm.

There is, however, one important exception to this conclusion. I am highly impressed with the extent to which businessmen accept sales revenues as a goal to which profit considerations are subordinated. It is not unusual to find a profitable firm some segment of whose sales can be shown to be highly unprofitable. For example, we have encountered several firms who were losing money on their sales in markets quite distant from the plant, where local competition forced the product price down to a level which did not cover transportation costs. Another case was that of a watch distributor whose sales to small retailers in sparsely settled districts were so few and far between that the salesmen's wages were not made up by the total revenues which they brought in! When such a case is pointed out to management, it is usually quite reluctant to abandon its unprofitable markets. Businessmen may consider seriously proposals which promise to put these sales on a profitable basis. There may be some hope for the adoption of a suggestion that a new plant be built nearer the market to which current transportation costs are too high, or that watch salesmen be transferred to markets with greater sales potential and that a mail-order selling system be substituted for direct

[2] This easily explained piece of magic thus did the apparently impossible in reconciling the low inventory desires of the firm's finance division with the maximum sales volume goal of the sales department.

[3] Perhaps, it may be surmised, because these competitors are also full cost pricers.

[4] This case illustrates the absurdity of the argument that marginal reasoning should not be used because data are incomplete. In fact, as in this case, the approach can on occasion cope with problems where lack of information might otherwise have been very serious.

selling in sparsely populated regions.[5] But a program which proposes explicitly any cut in sales volume, whatever the profit considerations, is likely to meet a cold reception. In many cases firms do finally perform the radical surgery involved in cutting out an unprofitable line or territory but this usually occurs after much heart-searching and delay.[6]

However, the desire to promote sales revenues rather than profits is subject to an important qualification which is illustrated by the attitude of a manufacturer of a new synthetic yarn who was reluctant to promote sales by introducing his product at a price which would not cover the cost of initial low volume outputs. Indeed the firm's usual rate of return on investment played an explicit and fundamental role in these deliberations. Management made it clear that it was not anxious to obtain profits higher than this. That is, once this minimum was achieved, sales revenues rather than profits became the overriding goal. But profit considerations did nevertheless play their role. And, of course, this must be so — if sales regardless of profits were the businessman's suicidal objective he would find it expedient to offer a full line of loss leaders at least for the short period before the receivers took over the enterprise.

To summarize, my own operations research experience indicates that businessmen frequently make decisions which violate the rules of marginal analysis, but that, with some exceptions, they are very willing to learn. Indeed, operations research may even *make* the marginalist's case: should the use of operations research methods in business continue to grow as it is growing now, the businessman's decisions may be expected to approximate ever more closely those which the marginalists attribute to him!

We must be careful however, in stating this conclusion, to distinguish between the marginalist's tools and the objectives which he sometimes attributes to businessmen. As has been stated, management seems typically to attach greater importance to sales volume than to profits. Especially where there is separation of ownership from management, characteristically the executive's aim seems to be to maximize something like the firm's market share or the rate of growth of its sales volume subject to the constraint that profits do not fall below a vaguely defined minimum level which is considered to be sufficiently high to provide funds for expansion and to yield to stockholders a return large enough to prevent dissatisfaction. There is nothing inherently less rational in such an aim than in profit maximization. And the tools of the marginal analysis, including the differential calculus and programming, can assist in the pursuit of either goal. My experience suggests that the businessman is willing to behave in accord with the marginalist's description in that he is delighted to accept the help of the marginalist's tools; but he prefers to stick to the ultimate aims which he has chosen for himself.

Now I turn very briefly to a totally different area — to monetary theory — to illustrate how the *tools* of operations research can help the theorist. It will be remembered that Keynes in his *General Theory* implies that the demand for cash for transactions and precautionary purposes will respond to changes in expenditure levels, but that these demands will be relatively interest inelastic.[7]

It is, of course, possible to accept these assertions as assumptions, but usually on such questions the theorist prefers to probe somewhat more deeply by investigating whether there is any motivation for cash users to behave in this manner. Cash is kept not for its own sake but because it helps the consumer and the businessman to carry on activities. To investigate Keynes's assertion we must therefore ask whether there is some way in which an optimum cash balance can be computed and, if so, how this optimum cash balance figure will be affected

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[5] Even this suggestion was not adopted by the watch distributor.

[6] Yet it is easy to show that a good marginalist approach even to sales revenue maximization requires abandonment of disproportionately *unprofitable* sales segments. The firm's resources should be allocated among its different products, territories, etc. in such a way that the marginal *profits* from all types of sales are equal. For if the sacrifice of a dollar of profit can produce a greater addition to sales of type *A* than to sales of type *B*, then sales cannot be at a maximum (subject to the profit constraint which is described in the next paragraph of the text) and effort should be transferred from the promotion of *B* sales to the promotion of sales of *A*.

[7] J. M. Keynes, *The General Theory of Employment, Interest and Money* (New York, 1936), 196–97.

by changes in incomes and interest rates. These are obviously important questions for business management, as well as for economic theory.[8]

The operations researcher possesses a well-developed body of techniques for determining optimum inventory levels.[9] These can be applied to our present problem, since a firm's or an individual's cash balance can be interpreted as an inventory of money which its holder stands ready to exchange against his purchases.

The fundamental result of such an analysis is that the optimum transactions cash balance will increase when the volume of the firm's business or the transactions cost of acquiring interest bearing securities (which I shall call the brokerage fee) increases and this cash balance decreases when the interest rate increases. But these are not proportionate variations. For example, the optimal cash balance increases (approximately) only as the square root of the volume of business transactions — i.e., there are economies of scale in the firm's optimum cash balance.

Intuitive reasons for this result can be suggested. First we note that the businessman is not limited to a unique cash balance by the volume of payments which is to be met. A payment of $80,000 to be made at even intervals throughout the year can be met by keeping the entire $80,000 on hand, or by investing it and withdrawing $40,000 after six months, etc. Clearly, if the brokerage fee goes up it will pay to cut down the number of withdrawals, i.e., the optimal cash balance will rise. Similarly,

if the interest rate goes up it will pay to make withdrawals as small and as late as possible, i.e., the optimal balance of idle, non-interest earning cash will fall.

We conclude that if firms are efficient profit maximizers, Keynes was probably wrong in de-emphasizing the influence of the interest rate on the transactions demand for cash. However, the analysis provides a firmer foundation for his view that the demand for cash should increase with the volume of transactions. But why should they not increase proportionately (as Keynes suggests)? The answer is again intuitively easy to grasp. The minimum broker's fee is what makes it unprofitable to take cash out of investments in frequent small driblets, although doing so permits cash to remain invested until the last possible moment. But the larger the amounts involved the smaller, relatively speaking, will be the minimum brokerage costs. On a thousand dollar bond purchase, minimum brokerage fees can be prohibitive. On a million dollar transaction they are negligible. Hence, the larger the total amounts involved the less significant will be these brokerage costs, and the more frequent will be optimal withdrawals. For this reason optimal withdrawals and cash balances will rise when the volume of transactions per firm increases, but will rise less than in proportion with the real volume of transactions payments.

We see then that operations research can contribute important results to the work of the economic theorist in a number of ways. But here is a two-way street. The debt of the operations researcher to the economic literature is no less considerable. Indeed this is one of the cases, whose number is unfortunately smaller than is often hoped, in which interdisciplinary cooperation has clearly paid off.

[8] The material which follows is based on my article, "The Transactions Demand for Cash: An Inventory Theoretic Approach," *Quarterly Journal of Economics*, LXVI (November 1952). See also James Tobin, "The Interest Elasticity of Transactions Demand for Cash," this REVIEW, XXXVIII (August 1956).

[9] See e.g. Thomson M. Whitin, *The Theory of Inventory Management* (Princeton, 1953), ch. 3.

IV. ECONOMICS, MANAGEMENT SCIENCE, AND OPERATIONS RESEARCH
Martin Shubik

In a survey of 149 companies using operations research, the American Management Association disclosed recently that 2.4 per cent of

the operations research personnel gave economics as their major background whereas 42.3 per cent came from engineering and 27 per cent

from mathematics and statistics.[1] This being so, it would seem that the role of the economist is a minor one. The field of operations research is apparently the happy hunting ground of the mathematician, the scientist, and the engineer. It is my belief that the reason for the relatively low level of participation by economists can, to a great extent, be explained by the originally military nature of operations research work. Furthermore, we can indicate why there should be a trend toward an increasing participation by economists as the uses of operations research techniques are turned more and more to the study of strategic problems of the firm and management science, and less to military, technical, and tactical applications.

The history of the development of operations research in the military has been briefly indicated by Hitch in this symposium. We can see how a concern with damage exchange rates, convoy sinkings, task force "effectivity," and so forth brought the over-all economic problems of measure of effectivity to the fore. The importance of the central economic problem becomes even more marked when operations research techniques are used to assist in the study of the firm.

The tendency in operations research work has been to construct many specific models for a great variety of individual problems. There are many examples of inventory studies, linear programming applications, systems engineering and systems design problems which have been treated by operations research groups. A specific inventory application may have as large and as direct a payoff to a firm as the submarine search problem payoff had to the military. However, if this were all, operations research techniques and practitioners would have a future little different from that of quality control experts. (Perhaps, at this point the term "operations research" needs defining; for, if it is taken as a "primitive concept," by some it is equated with all that is good and by others with all that is bad. Our contention is that operations research *techniques* and applications have helped to open up the path by which a *theory*

of the firm and a science of management can be constructed.)

The economist is schooled to think in terms of, and deal with, multivariate systems. He understands that there may be great difficulties in determining criteria to measure items such as productivity, efficiency, long-range profit, capacity of the firm, and so forth. He is well acquainted with problems of *evaluation* and the implications of the concepts of marginal or incremental cost and of alternative cost.

Operations research teams, on the other hand, are strong in the ability and willingness to devise measures for the multivariate systems, but on the whole those trained in applied or pure science appear to have less appreciation for the evaluation problems than do economists.

Unfortunately the ability and interest of economists in dealing with multivariate systems have been severely limited to the conceptual and "conversational" levels for the most part. The operations research teams with their measurement techniques in the past few years have rushed in where the economists have feared to tread. The proliferation of work on inventory theory is an example. Features such as the cost of changing production and many specific handling and carrying charges have made their way into both the literature of operations research work in the firm and into modern economic theory, without being dismissed by the latter as a "minor friction" scarcely worth noting in a general theory.

Stress has been laid upon the economist's ability to handle abstractions. Many individuals automatically assume that this ability belongs to the mathematician far more than to the economist. When dealing with the type of economic model-building required for the study of business systems, this is not so. The creative part of model-building, the ability correctly to formalize and to understand the basic features of the problems in such a manner that an abstraction can be formulated and a model built is more naturally the job of the economist than of the mathematician. Once the problem has been well defined the role of the economist is diminished and the importance of the mathematician and others is increased. A mathematician may be far more adept at solving an inventory equation than an economist, once it

[1] "American Management Association Survey of Operations Research Activity," American Management Association Operations Research Orientation Seminar, May 1957.

has been formalized; however the economist may have a far greater ability for recognizing that the equation at hand is relevant to the problem. Furthermore, he may observe that it is analogous to other equations which arise from seemingly different economic problems which beneath the surface exhibit great similarities.

Specifically, the interaction between the economist and others involved in the study of the problems of business systems comes in the design of *models*, the understanding of the *logic* of the business system, and the *evaluation* of the measurements made. Much of economics has been "theory without measurement"; a great amount of operations research has been "measurement without theory." The blend of theory construction, model-building, measurement, and testing at the level of the firm offers economists and managers an opportunity to increase considerably their understanding of the theory of the firm.

Perhaps the greatest shock that awaits the academic economist who works with industrial organizations is the realization of the divergence between the simplifications of current theory and the complexities of reality. In many ways this presents a great challenge to the economist. Even though the theories he has studied may be vastly distorted when matched against reality, at least he has been taught how to think about the firm abstractly as an entity. He will find that most engineers think about the firm in terms of manufacturing or engineering; similarly those whose training has been primarily in marketing look upon the firm from a different restricted viewpoint. It is the task of the economist to examine and understand the complexities without getting bogged down in detail or side-tracked into a distorted view of the firm.

The three examples, given below, of work containing a blend of economics, business system analysis, and operations research techniques illustrate both the "tactical" and "strategical" aspects of the work. The first, which deals with bidding, illustrates the relationship between two sections of a firm: marketing and manufacturing. The second, on inventory theory, is apparently limited to one section of a firm's activities; however, economic analogies show that the analysis has a more general use. The third example deals with the firm as a whole and some of the basic implications of this work for the economist and manager are drawn.

I

Problems of competitive bidding and auctions have long been the delight of the esoteric group of economists who have studied price theory and its history. Institutional studies of auctions and "rings" and mathematical economic models of bartering, bargaining, and exchange have abounded. Recently new impetus has been given to the study of bargaining by the work in psychology and game theory on different types of decision-making.

A problem relating closely to the literature on bidding and related to problems of productive capacity comes about in the following manner. Suppose that a firm is submitting bids on a number of government contracts. Suppose furthermore that the plant has a limited manufacturing capacity. If the firm wishes to maximize short-term net revenue it should pursue a policy of pricing such that it attempts to obtain as high a price as possible on the contracts it receives at the expense of increasing its risk of losing some contracts, taking into account that owing to capacity limitations it is unable to handle all contracts.

A highly simplified model of the type of problem noted above illustrates the connection between pricing and capacity limitations. Suppose that a firm is bidding on n identical contracts. It has manufacturing facilities which enable it to handle only k contracts. It must use the same bidding price for identical contracts. We make a series of heroic assumptions that we can relax later. The variable costs of production are zero. There is a fixed cost C which will be paid regardless of utilization of plant. If the firm obtains more than k contracts it must relinquish the extra contracts. This can be done costlessly. Let the probability that the ith contract is let for a price p or less be represented by the cumulative distribution $\Psi_i(p)$. The probability that the firm puts in a successful bid is $1 - \Psi_i(p)$. On the assumption that the probability of getting any contract is the same at the same price, we can write

$\Psi(p)$ to represent the probability that any contract is let for a price p or less. We assume that the firm wishes to maximize the expected value of its short-run net revenue. In order to do so it must choose its bids to maximize the following function:

$$R = p \sum_{i=0}^{k} \frac{n!\,i}{(n-i)!\,i!} (1 - \Psi(p))^i (\Psi(p))^{n-1} +$$

$$pk \left[1 - \sum_{i=0}^{k} \frac{n!}{(n-i)!\,i!} (1 - \Psi(p))^i (\Psi(p))^{n-i} \right] - C.$$

In order to convey an impression of the importance of the capacity limitation a simple example is computed below. For this example we have assumed that $\Psi(p) = p^2$. We consider the case where the firm has only sufficient capacity to handle two contracts. We can immediately derive the explicit form for the expected revenue which is:

$$R = 2p - np^{2n-1} + (n-2)p^{2n+1} - C.$$

From this the price has been evaluated for the optimal bid of the firm as a function of the number of contracts available. This is given in the table below.

n	1	2	3	4	5	∞
p	.572	.572	.637	.522	.720	1

As is to be expected, if the number of contracts available is at or below the capacity of the firm, the strategy will be uninfluenced by the number of contracts if the awards are statistically independent. Even if all contracts were obtained the capacity limitation will not affect profits. The expected revenue from the contracts equals the sum of the expected revenues from each considered separately. As soon as the number of contracts available is greater than the capacity of the firm the expected revenue is influenced by the capacity limitation, as is indicated by the truncated binomial expression for the revenue. In this example the optimum bid first falls, then rises, and approaches an asymptote of $p = 1$ as the number of contracts increases. The price range has been normalized so that price appears as a number between 0

and 1. This was unnecessary, but in such a form the computation is lessened.

We do not intend to present a detailed analysis of this bidding problem now. However some comments concerning the difficulties of formulation, the removal of simplifications, and further results from analysis should be noted. Most of the simplifications can be removed at a cost of greater computational difficulties, although the nature of the problem remains.

1. If variable costs are a constant, say b, the formula is modified by replacing p by $(p - b)$.

2. The "loss function" assigned to failure to obtain sufficient contracts, or to obtaining too many, was very simple. It is evident that in some situations where a firm is striving to build up good will for the possibility of future military contracts or other business some cost should be assigned to the inability to fulfill a contract or to the ill-will caused by submitting too high a bid. In general, these figures are not obtained easily from *a priori* considerations.

3. If the contracts are very different, the binomial expression, which can be approximated and evaluated, must be replaced by a multinomial expression, and a realistic problem will involve the selection of n different bidding prices.

4. Even when the contracts are the same, if they are more numerous than the capacity of the plant permits, the optimal price strategy may not involve equal prices. If, when too many bids are successful the firm has the choice of selecting which bid to reject, the bidding strategy may call for more than one price. A simple example illustrates this. Consider the same distributions as in the previous example and let $k = 1$, $n = 2$, p_1 is the bid on the first contract and p_2 is the bid on the second. Say $p_1 \geq p_2$ (this entails no extra restrictions on the problem). For an optimum profit the conditions which must be satisfied are:

$$\frac{\partial R}{\partial p_1} = \frac{\partial R}{\partial p_2} = 0,$$

where:

$$R = p_1(1 - p_1^2)p_2^2 + p_2(1 - p_2^2)p_1^2 + p_1(1 - p_1^2)(1 - p_2^2) - C.$$

The third term indicates that if both contracts were obtained, the first would be accepted. The

solution of the example yields $p_1 = .757$ and $p_2 = .707$.

The actual computation of more or less complicated "realistic" models of this type of bidding situation can be handled. It is possible to derive graphical methods and rules-of-thumb for operating personnel. Leaving this aside there are two aspects of importance to the economist which arise from this model. The first is an extension of knowledge about optimization under capacity limitations. This is of considerable importance in explaining the nature of the relation between manufacturing and marketing and the reasons why "to get as much business as possible" is not necessarily the optimal policy for the firm. The second feature concerns the interpretation of the model. We have been talking about a problem involving a manufacturing firm bidding for contracts. A formula very similar to the one derived for handling this problem has been used by Martin Beckmann for the study of airline reservation problems.[2] The capacity limitation is the capacity of the aircraft; the available contracts are the passengers available; the pricing problem has to do with the obtaining of customers and the handling of the "no-show" passengers. The ability to draw analogies between apparently different types of problems encountered in the economic life of the business world is probably one of the most important contributions that an economist can make to an operations research group working in a business. By drawing the correct analogies not only is the scope of various techniques broadened but the actual backlog of facts and instances useful to the verification of economic theory is extended.

II

The next example comes from inventory theory. Much of the basic work in the development of modern inventory theory has been done by economists and mathematicians. They have investigated the relationship between lot size and various carrying and purchasing costs as well as the effect of uncertainty on inventory levels.

The square-root formula for optimum lot size is easily derived from simple economic considerations and has direct use both as an applied

tool of operations research and, as will be noted below, in economic theory.

Let Y represent the expected number of yearly sales, let Q be the size of the lot purchased, and C be the unit cost of an item purchased; S is the procurement expense incurred in placing one order; I is the carrying cost expressed as a percentage of procurement cost. This latter includes interest charges, depreciation and obsolescence charges, etc. Then the total variable costs for the period under consideration are:

$$\frac{QC}{2} I + \frac{Y}{Q} S.$$

The first term in the formula gives the average carrying charges for the period, based on the assumption that inventories are allowed to fluctuate from zero to Q. The second term gives the procurement expenses Y/Q, the number of times during the period that orders are placed; and this multiplied by S gives the total procurement expenses. Upon differentiating the above expression with respect to Q and setting the resultant expression equal to zero, we obtain the optimum lot formula:

$$Q = \sqrt{\frac{2YS}{IC}}$$

This expression was derived in order to study the optimum lot size for procurement of stock. The analogous problem arises in the determination of optimum lot sizes in manufacturing. The implications of this analysis for operations research and for economic theory do not end at this point. Baumol[3] has applied essentially the same considerations to investigating the transactions demand for cash.

In another part of inventory theory economic analogies are plentiful. Basic considerations of uncertainty have led to the investigation of safety allowances to protect against running out of stock when faced with a fluctuating demand. It has been shown that for the type of random fluctuation which can be reasonably expected in many situations the safety allowance should be varied with the square root of the size of

[2] M. Beckmann, "An Airline Reservation Problem" (mimeographed).

[3] W. J. Baumol, "The Transactions Demand for Cash: An Inventory Theoretic Approach," *Quarterly Journal of Economics*, LXVI (November 1952).

expected demand.[4] This result is of considerable relevance to business cycle theory.

An analogous problem to that of safety allowances in inventory position arises in the consideration of labor supply for project work. Here the inventory is that of skilled personnel, the expected demand is the expected loss of personnel, and the loss function is the measure of the damage caused by an inability to complete a project due to lack of personnel or the added costs of carrying surplus individuals.

III

The last example deals with more general aspects of the theory of the firm. It has been evident for a considerable time that the crudely simplified model of the behavior of the firm, in which the individual entrepreneur maximizes a well defined short-term or even long-term revenue function, is of highly limited use in explaining the functioning of firms in our economy. There are few individual bachelor, owner, entrepreneurs running small single-product firms as unincorporated enterprises in our economy. The facts of life force us to recognize the presence of large multiproduct firms operating in a welter of different market structures, under a host of social and legal restraints, operating under several types of institutional forms such as corporations or partnerships, owned jointly by many individuals and often controlled by men who are either fractional owners or have no capital invested in the firm they control. All this does not necessarily mean that we should abandon our attempts to apply the rigor of the methods of economic theory and fall back upon pure institutionalism. It simply means that the job of the economist becomes more difficult and the degree of sophistication and willingness to deal in detail with multivariate phenomena becomes higher than it has been previously. The necessity for careful operations research work combined with some of the deepest considerations about the theory of the firm is evident when a research group attempts to investigate the goals and the capabilities of the firm as a whole. An understanding of the payoffs and the strategies available to a firm is

[4] T. M. Whitin, "Inventory Control in Theory and Practice," *Quarterly Journal of Economics*, LXVI (November 1952).

basic to the eventual construction of a modern theory of the firm and the market. Dividing difficulties, we present a brief sketch of the type of work which can be done on both of these topics.

Many of the statements concerning corporate optimization policy are ill-defined and vague. In this manner they can be given scope to encompass the broad considerations of the society in which a corporation thrives. Nevertheless, from some operational viewpoints, it is necessary to be able to make more concrete statements about the objectives of the firm with regard to long-term and short-term profit making, plans for growth, and the degree of risk under which it will operate. One way in which this can be done is by introducing a more detailed model of the financial features of the firm into our theory than has been done previously.

An explicit consideration of the corporate account and a separate stockholders' dividend or withdrawal account makes it possible to express mathematically many of the various goals of manager-run corporations, unincorporated individually owned and operated firms, and other business forms. The present concern with defining economically useful and operationally meaningful "objective functions" for mathematical programming lays emphasis on this interlinkage between the analysis of economics, the techniques of operations research, and the needs of modern management.

Possibly one of the most promising areas of development which may prove to be of basic importance to all interested in any aspect of the modern firm is *simulation*. A simulation of a system is a *model* which can be used for experimentation so that prediction of the effects of changes in internal or environmental conditions can be made. Simulation techniques are, in general, concerned with the portrayal of multivariate systems for the purposes of manipulation. The simulation may utilize a digital computing machine or may be an analogue. The idea of building analogues is not new to economics; hydrostatic and electrical circuit analogues have been used for teaching purposes. They appear to be a very effective tool for illustrating the nature of the interlinkage between many variables in a complex system.

The aircraft industry has used both digital computing machine simulations and analogues for some time with a great deal of success. The building of models to study complex technical and man-machine systems for engineering design, civil defense problems, military task force systems, the design of irrigation and other complex systems has already indicated many of the uses of this approach.

The mere portrayal of the flow diagrams needed before any actual simulation can be run is in itself of primary importance to the manager, economist, and operations researcher. The construction of the diagrams forces explicitness and a degree of definition that cannot be obtained by verbal methods. Furthermore the flow diagrams are sufficiently flexible that connections and interrelationships may be portrayed without immediately writing down mathematical expressions which can restrict the realism and the use of a model if specified too early.

The labor involved in constructing a simulation serves as an organized program for studying the system as a whole. A considerable appreciation and understanding of a complex system as an entity can be obtained in this manner even if no final computations are performed.

Until recently most of the work with simulation has been slanted toward complex technical systems. Human behavior has appeared to be too difficult to portray. When, as economists, we think about the theory of the firm and its relation to its subject matter, we realize that we know very little about the complex man-machine system that is the modern firm. We may not, as yet, be able to write down equations to predict many of the relevant behaviors in an economy, but we are in a position to construct models to explore the relationships between the different parts of systems internal and external to the firm.

The manageability of the modern firm is coming to depend more and more on the ability to think logically and strategically about complex entities. Success or failure in a market may hinge upon the estimates of the effect of a change in one or two parameters. These estimates are made daily by businessmen who are forced to do so in the course of their operations. Usually the estimates are made implicitly and are only reflected in the actions taken. They are not available to help other managers or economists to understand the firm. Application of the techniques of simulation promises to improve matters for both the economist and the manager, by providing a unified logical framework in which much information is made explicit and is recorded and synthesized in the study of the firm as a whole.

In some cases it appears possible to estimate the forms of the various relationships encountered in simulations of the firm. Then computation is possible and the simulation can be used to explore both qualitative and quantitative features of a system. The sensitivity of the system to parameter changes can be investigated, and its viability in face of varying conditions can be studied.

The theories of the firm, of oligopoly, and of organization all have suffered owing to the vast over-simplifications made, due to our inability to handle multivariate systems. Management in our present economy is beginning to suffer for the same reason. Work in the handling of more sophisticated models of the firm calls for joint participation by economists, managers, operations researchers, and others. The promise for economists is in better theories and the promise for the firms comes in having economic theories which are worth using.

We have briefly surveyed some of the relations between economics, operations research, and the general area of business systems analysis. Stress has been laid upon the importance of the role of the economist as a generalist and a model builder whose aim is to search out the structure of a business system and to explain its consistencies or locate its inconsistencies. In so doing the economist may contribute considerably to the evaluation of operations research and the advancement of work on business systems. At the same time the debt of the economist to his other colleagues grows, for he is provided with new information, methods, and the opportunity of studying the validity of many of his theories.

V. COMMENT
T. C. Schelling

Whatever it is — and the definitions provided in the papers collected here are rather diffident — operations research is not what it used to be. The frontier has been closed; pioneers have given way to professionals; exploration has given way to development. The development — assuming that the papers collected here *are* about operations research — seems to be following two main lines.

One is the cultivation of operations-research technique and an operations-research orientation by economists and other professional groups. The techniques are those of quantitative analysis, with a strong flavor of probability and uncertainty; the orientation is toward practical decisions and programs, often with a real or vicarious client relationship. In economics this is close to, or perhaps just is, applied econometrics; to the extent that sound quantitative analysis is superseding unsound non-quantitative analysis, it is nearly coincident with applied economics. I think it may be worth while to consider whether it is helpful or confusing to keep the term, operations research, for this kind of activity, once it has accomplished its missionary work and ceases to be a distinctive kind of economics. But whether or not we use the term, the growth of this sort of professional activity, well described by Professor Baumol, is an important phenomenon in the science and profession of economics, both here and abroad.

The other line of development is the professionalization of operations research, with a "supply of persons trained for operations research as such" beginning to appear, as discussed by Cooper and Shubik. Undoubtedly much of what is called professional operations research could as well be called applied economics, applied psychology, applied military science, and so forth; nevertheless, there is a core of technique and methodology, suggested by terms like "systems theory," oriented toward the similarities and analogies of quantitative problems from widely different fields, that represents not an "applied" discipline but rather the theory and practice of "applying" disciplines, any disciplines. It is this line of development, rather than the increased application of particular sciences to practical problems, that has the more legitimate claim as spiritual descendant of what used to be called operations research.

I recall the characterization of operations research in the "classic work" of Morse and Kimball that Mr. Hitch refers to, published in 1951 and relating mainly to the wartime adventures of operations-research pioneers. It was distinctly *not* the application of economics by economists, or of psychology by psychologists, or of electronics by electronics engineers and physicists. In the conception of a decade ago, operations research had a distinctive *amateur* quality about it. Imaginative people, scientifically trained for the most part, who enjoyed solving puzzles, discovered unexpected uses for their talents and insight. Morse and Kimball made a special point that operations research was not then the application of *science* to military operations, but the application of *scientists* to new problems unrelated to the content of their original training and experience. They not only emphasized the striking success that, say, an imaginative biologist with a slightly mathematical bent might achieve in the field of submarine tactics; they even implied that some of the success was due to the very freshness of view that an amateur brought to the problem, to the fact that the non-specialist was not encumbered with the settled views and working methods of experts who had been too close to the particular problem for too long.

This original kind of operations research cannot constitute a "field" or "profession"; it is rather a *stage* in the development of an applied field, as exploration is in the development of new territory. It was the virgin nature of the problems, problems that had not yet been systematically attacked on a scientific level, that made it possible for amateurs to discover enormously superior approximate solutions. But if

the amateur stays around long enough, he ceases to be an amateur and becomes a traffic expert, a submarine-tactics expert, an inventory expert, or something of the sort. He may be doing operations research as defined by Mr. Hitch, "the use of systematic quantitative analysis to aid in the making of management decisions," but that may mean only that he is making the best of recent advances in his own applied field.

I mention this not just to quibble over the term, nor to deny that what is presently called operations research is as worth while, whatever its name, as the earlier kind of activity was, but to try to identify a professional trend in economics. It makes sense to identify some economists, or some of the members of any other professional group, by a particular technique that they use or a particular interest as long as it is a specialized minority of the profession to which the term is applied; economists who used to know some mathematics or statistics were called "mathematical economists," "statisticians," or "econometricians." But once a technique, if it is a good one, becomes fairly general in the profession, particularly if it is something as unobjectionable as "making the best use of available quantitative data" or recognizing the multivariate nature of problems that are really multivariate, it ceases to be helpful to identify the technique as anything more than one of the techniques of the profession. To judge from some of the examples given by Baumol and Shubik in the papers collected here, numbers of graduate students in economics have been writing dissertations that were "operations research" without knowing it.

This does not mean that Baumol is just discussing *economics* in his paper. He is discussing the striking phenomenon of large and growing numbers of economists getting directly involved in the problems of business. This is still a distinctive activity; but from the examples cited it appears to me that most of what they are doing can be called economics. It is the application of economics to business problems, and the influence of this activity on economics itself, not a specialized activity called "operations research," that we are witnessing.

Linear programming, to take an example, is often characterized in terms of the linear nature of the model; but it is really the *programming* orientation that typifies what has happened in the economics profession. The marginal-cost controversy that Baumol discusses took place in a very different context from that of today. Economists were interested in whether or not it was valid to *assume* that businessmen, consciously or not, maximized profits successfully, and hence valid to use that assumption for drawing generalizations about business behavior. And the issue for the most part was not whether businessmen had the intellectual acumen and access to data necessary to maximize profits, but whether they were sufficiently sophisticated to see the problem correctly and to use the intelligence and the data that they had.

Our present era, in contrast, is one which economists are more and more impressed with how difficult it may be to maximize profits, and more and more concerned to find ways of helping. I am not persuaded by Cooper and Shubik that business problems have become "increasingly complicated" decade by decade; but certainly economists are more preoccupied now with the problem of how to maximize profits than they were in the 1930's.

The profession has made several important discoveries. One is that economic theory, or perhaps I should say the analytical apparatus and talents of economists, can be of appreciable help to business. A second is that working with a business firm can be fun. A third, which I think is rather well brought out in the papers collected here, is that the economist can not, in a business any more than in government, play the pure role of expert adviser who abstains from political or business judgments, avoids the administrative process, answers only the questions put to him, and confines himself to identifying the alternative consequences that go with alternative decisions. He must usually participate. He must usually formulate the questions themselves for his client. He must make his own value judgments if the executive bureaucracy of the firm or agency is incapable of articulating the value judgments for him.

It is interesting, too, how some of the most applicable types of analysis have been the most esoteric. The "purest" theory has often become the most marketable. And a new turn can often

be given to the student's proverbial question, "If you are so smart, why aren't you rich?" Some of our folks are getting rich.

Not all of this, though, came out of operations research. Much the same thing occurred in a quite separate field, one that did not originally attract the more mathematical among the theorists — the development of backward countries. There, too, the economist has found that to expound a rule — say, for allocating investment in accordance with comparative advantage — may only raise the next question, How to do it? A prescription to balance the volume of current investment against a national time-preference discount rate is unhelpful; the economist may have to make up his own rate of time-preference, if nobody else can articulate one. So in several fields we have become a how-to-do-it profession, with the discovery that "it" is awfully hard to do, but also the discovery that economists can sometimes help.

There is another striking similarity between the recent preoccupation with economic development and the preoccupation with military planning. It is that economists are once again deeply concerned with the most traditional part of economics, the allocation of scarce resources. And although there are some powerful modern analytical techniques to help in the calculation of alternatives, it is Mr. Hitch's contention that at the very highest levels of strategic planning what is most urgently needed is not more and more elaborate quantitative techniques for multivariate analysis and computation, but rather the most rudimentary grounding in elementary economic principles.

Mr. Hitch says that "an integral aspect of the requirements approach [in military planning and budgeting] is that cost considerations are excluded until after the requirement has been established — by 'military judgment' and 'on the basis of needs'." I am personally convinced that he does not exaggerate. As he makes clear, there are at least two dimensions in which costs are ignored except for an ultimate test of feasibility or acceptability. One is in the *composition* of a program, i.e., the distribution of resources among the component parts. The second is in the *level* of total effort. "Requirements" in the latter sense are seldom stated in terms of a functional relationship between the total outlay and the probability of achieving a stated objective; there is rather a single "requirement" that presumably denotes some proper, reasonable, but unexpressed degree of probability. As Mr. Hitch makes clear, there can be a good deal of implicit relative-cost analysis and implicit judgment about what degree of risk is reasonable in the light of budget and tax considerations. The point is not that economically rational marginal adjustments do not get made; it is that they get made by a process that is more implicit and intuitive, less straightforward and decisive, for not being overtly recognized as legitimate and essential at the highest level of decision.

This raises the question, which torments so many economists whose work brings them in touch with military planning and procurement, how to indoctrinate the planning and budgeting processes of the defense establishment with the elementary economics of choice.

I think the answer to this is in two parts. One is that just a little knowledge of economics on the part of a modest number of people in the defense establishment will not do the trick. The bureaucratic *system* has to be gradually redesigned — in the words of Mr. Hitch — "to relate criteria at decentralized decision levels with *incentives* and *information* at the same levels." True, the required budgetary and organizational reform has to be comprehended and appreciated at least by some of those at high levels in the organization whose initiative and determination are needed to accomplish it; but their comprehension and appreciation alone are not enough. One has to recognize the defense establishment — and, of course, the whole federal government — as an *economic system*, and try to design it so that there are incentives and capabilities for making efficient economic decisions at a multitude of decentralized points.

The second part of the answer is concerned with indoctrination rather than with design; with getting across that "elementary lesson" in economics. And here I think Mr. Hitch has pointed toward an important pedagogical principle. It is that the lesson may get across, and in fact is getting across, less through simple, straightforward exhortation than by an indirect, difficult, and comparatively complicated route. That route is "operations research." And I

think there is an analogy here between the ultimate influence of economic reasoning on military planning and the gradual influence that economists are achieving in some types of business planning.

In the first place, it is easier to convince somebody that he has done less than a perfect job in making *complicated* decisions than to tell him that his fundamental principles are unsound, that he misconceives the simplest problems of choice. Operations research, dealing with specific problems on which the client is aware of his need for help, can be an elementary lesson in disguise, a means of drawing the client into intellectual participation in the application of economic reasoning.

There is another reason why operations research is a good medium through which to reach the heads of business and the heads of military establishments. It gives the economist some credentials. Instead of posing as a mentor or intellectual kibitzer he can pull out a slide rule and save the man a little money, and show that there is a practical value to his way of looking at problems. Lawyers, I think, serve well in the higher echelons of business and government be-

cause of intellectual qualities and training; but they get started toward the top because there is a particular kind of problem, legal problems, that they can solve better than other people.

Discussion of this topic is almost bound to sound patronizing towards the military establishment; the patronizing tone needs to be counteracted. Fortunately, there is evidence in the papers collected here that military planners compare well with the other professions. Heads of business too, according to Baumol, can make mistakes that look egregious to an economist; and perhaps the nature of their business gives them less excuse than the military planners. More than that, even the heroes of this symposium, the operations researchers, are accused by Mr. Hitch, with good illustrations, not just of making errors, but of making precisely the elementary error that has been attributed to military planners. So there is nothing in what I have said that in any way contradicts Mr. Hitch when, asking what lies behind military judgment, he answers, "Frequently, first rate intellectual capacity."

VI. MARGINALISM VERSUS ALGORITHMISM

Stefan Valavanis

ECONOMIC, engineering, and military problems boil down to maximizing one thing or another subject to constraints. In all cases [1] the marginal way of thinking gives a correct restatement of the problem, but not necessarily a convenient way to solve it. The issue cannot be whether marginal thinking is correct or desirable but whether marginal thinking leads to convenient solutions. Therefore, if operations researchers have veered away from marginal thinking, they have done

[1] Local maxima obtain only where MB (marginal benefit) equals MS (marginal sacrifice). "Marginal" usually means "occasioned by infinitesimal departures from the solution value(s) we are seeking." With mathematical discontinuities in the maximand and/or the constraints, the formula is $MB < MS$. If there are several local maxima, the last formula still holds, provided we reinterpret the word "marginal" to mean "occasioned by *any* departures, small or large, from the solution value(s) we are seeking."

so because it did not help to solve the particular problems they had to tackle. In this paper I shall suggest by an example why the marginal approach (as generally understood) is bound to fail as a method for actually finding the solution.

Three methods are available for maximizing under constraints:
1. The marginal method, generously interpreted (see footnote 1).
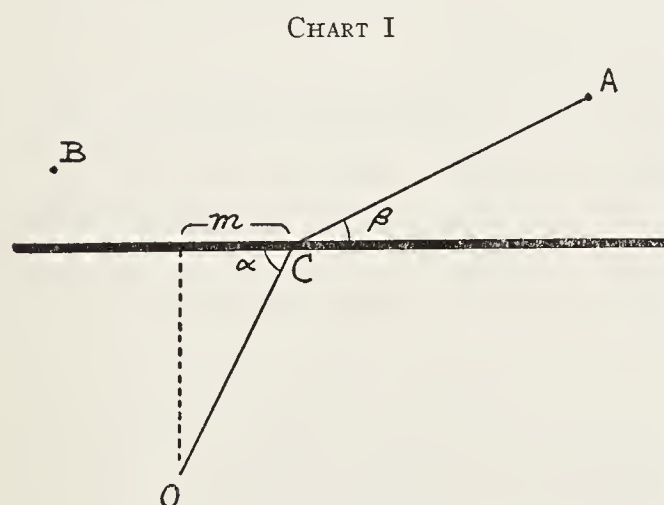2. Exhausting the alternatives.
3. Algorithms.

In practice, the *marginal method* is either too expensive computationally, or unintelligible to the customer, or both.

Exhausting the alternatives is, like the marginal method, formally correct. It is intelligible enough, but too expensive to inflict even on the most stupid customer.

In most maximizing problems, I think the ideal mentality is neither marginal-mindedness nor exhaustion-mindedness, but *algorithm-mindedness*. An algorithm is a rule that leads to the desired solution step by step; before the next step is taken it rules out all other alternatives; in each succeeding step the number of alternatives left untried shrinks in a systematic way because the rule makes sure all others are not worth considering. Algorithms combine marginal-mindedness with exhaustion-mindedness.

I shall illustrate the algorithms with a relatively simple traffic problem, whose marginal formulation is utterly hopeless as a guide to computing the solution. Imagine that my car is facing the Boston Public Garden at the corner of Commonwealth Avenue and Arlington Street. My next errand is for a certain piece of hardware that I can buy only on Bowdoin Square, or for the same price at a shop located at the corner of India and Milk Streets. Given traffic conditions, fuel cost, my time's worth, and disregarding where I want to go afterwards, (a) which shop should I buy from, and (b) by what route should I drive there?

A simple but nasty problem indeed. Exhaust all possible routes? This is a staggering job in any city, let alone Boston. The marginal analysis? Formally, the problem is a complicated case of Palander's law of refraction pictured in Chart 1. The horizontal line separates two
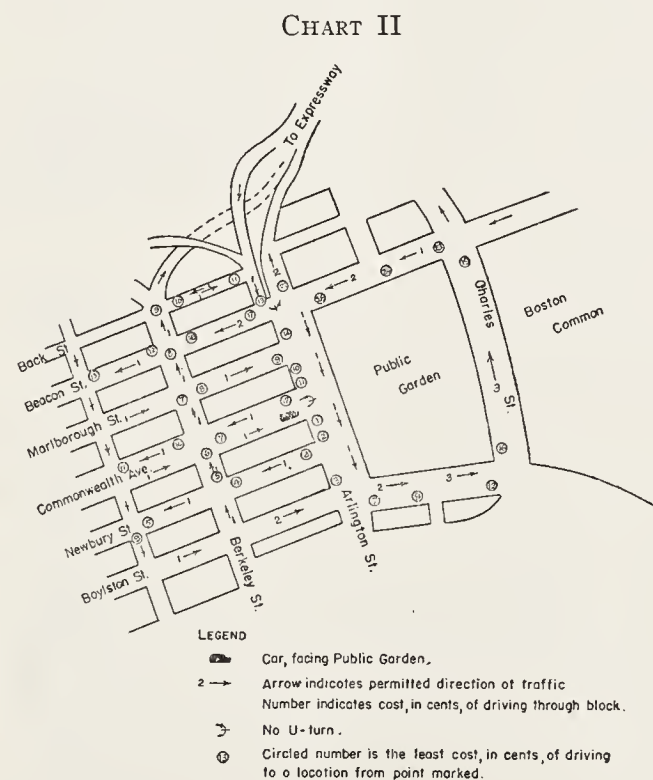
CHART I

areas of unequal (but constant in any direction) transport costs, O is the origin, and A and B are the rival shops. The best way to get to A, if one has to, is by a broken line through C where the cosines of α and β are inversely proportional to the ratio of unit transport costs. Some other broken path is cheapest if one has

to shop at B. Compare the total costs of getting to A and B and choose the cheaper. The cheapest *paths* from O to A and from O to B were already solved before we compared total costs.

The inverse cosine formula is easy to apply to paths through homogeneous media. But Boston's downtown area is tremendously complex. Not only does the cost of driving vary from street to street, but making a turn is more costly than straight driving, turning left more costly than turning right, U-turns and some left turns are illegal, and certain streets are one way.

If we were to express all this in a total cost function to be minimized, it would have to involve (1) the variables x, y that define which block we are trying to drive through, (2) the function $y = f(x)$ of the path we seek, (3) penalty for driving against traffic in a one-way street (expressed as $f'dx > 0$), (4) penalties for left turns (expressed $f'' > 0$), and (5) the coordinates of origin and destination.

The algorithmic solution. The map below shows the neighborhood of Commonwealth and Arlington. The uncircled hypothetical figures are

CHART II

LEGEND

🚗   Car, facing Public Garden.

2 →   Arrow indicates permitted direction of traffic Number indicates cost, in cents, of driving through block.

↷   No U-turn.

⊙   Circled number is the least cost, in cents, of driving to o location from point marked.

the costs of moving from one end of the block to the other in the direction indicated by the arrow. For certain blocks, it is cheaper to drive up than down. A single arrow means the street is one way. For simplicity let the cost of *any* right turn be 1¢, and of *any* left turn 4¢. This

convention does not make my method less general. How far can you drive for 1¢? You can just turn the corner onto Arlington. For 1¢ more you can drive to the corner of Arlington and Newbury. For still one more cent, you have a choice: you can either (a) make another right turn onto Newbury in an attempt to reach the Expressway or (b) drive on to the corner of Arlington and Boylston. A left turn from Arlington onto Boylston costs 4¢ more. If you had taken course (a), for the same additional 4¢ you could have moved along Newbury to Berkeley Street (1¢), made a right turn (1¢) to Berkeley, reached Commonwealth (1¢) and Marlborough (1¢). Figures in circles show the lowest total cost of reaching each location. In special cases there are two or several equally cheap routes for getting somewhere. We don't bother about the various *expensive* routes. The secret is to proceed marginally, one cent at a time. Begin at the origin and see how far 1¢ will get you; mark those points one. If the origin has adjacent points that cannot be labeled one, go back to it and see whether 2¢ can reach them. If so call them two; and so forth until all its adjacent points are labeled. Then go to the points marked one. See how far 1¢ would take you from there; call such points two. Somewhere along the process you reach your destination. This rule, or algorithm, exhausts every point P on the map, and attaches to it a unique number, V(P), which represents the lowest cost of getting there.

Every time we label a location P with its number V(P) we do not have to consider again how much it costs to get to it. Every time we have labeled with V(A), V(B), V(C), . . . *all of the adjacent points* A, B, C, . . . of location P, we stop asking how far we can get from P, at additional cost, and we concentrate on A, B, and C. In each step the algorithm reduces (faster and faster) the number of candidates among the alternative paths that lead from origin to destination. Instead of applying the marginal principle in grand style over all locations at a time, the algorithm in each step selects a small subset of them, and on this subset only performs a marginal comparison, viz., What is the next point that can be reached with a small increment of cost?

Here is a crude analogy to give a feeling for this algorithm: Imagine that where the car is parked a spring of thick lava or honey begins to flow. The uncircled numbers (and the cost — 1¢ and 4¢ — of making a right or left turn) are coefficients of friction telling how much resistance the flow of lava encounters. The tongue of lava cannot make a U-turn on Commonwealth Avenue, and can only flow right on Arlington Street. Depending on the quantity of lava, it will overcome the friction of Arlington and its side streets, and surround one block after another. Given enough lava, it will reach the threshold of one or another of the two hardware stores. When this happens, stop. That is the desired store. What is the best way to get to it from the origin? Go to the hardware store in question, and put a boat on the lava. Now let the spring suck back the lava; row the boat toward the retreating lava, noting down what streets you pass, until you arrive at the spring. You have charted the cheapest path to the best located store and the problem is solved.

Getting in the boat and paddling back can be restated as follows: Start from P, and examine merely its adjacent points A, B, C, etc.; choose the point, say C, whose value V(C) is less than V(P), but in such a way that V(P)-V(C) is greater than V(P)-V(A), V(P)-V(B) etc. If, however, V(C) = V(A) choose C if the cost of moving from C to P is less than or equal to the cost of moving from A to P. Then repeat the process from the point C toward its adjacent points Q, R, S, and so on, until the origin is reached.

This technique is analogous to a climber's following the sharpest gradient when he wants to go to the top of a single-peaked mountain by the shortest route.

You might say: But this algorithmic method does no more than plot the "isovecture" curves [2] around the origin. Precisely: the practically difficult problem is how to construct such curves when the transport cost field is not homogeneous, and to do so with as few trials and errors, corrections and readjustments as possible.

The algorithmic method compares very favorably with the marginal approach even in the extremely simplified problem I proposed. It works in more complicated cases. For instance, it can help us decide between three, four, or any number of hardware stores. It helps when the cost element is very complex, involving separate coefficients for mileage, time, idling.

[2] Lines representing equal unit freights to (and from) a place. See Tord Palander, *Beiträge zur Standortstheorie* (Uppsala, 1935) and August Lösch, *The Economics of Location* (New Haven, 1954, English translation).

Or, we may want to compare driving with other methods for getting to the store. Thus for about 80¢ one can go by subway — 10¢ to get to the parking lot south on Arlington, 20¢ for fare and 50¢ parking fee. Or, if we had a number of errands to do, and do not care about their sequence, the method will tell whether we ought first to buy hardware or groceries; and in which store. Having bought the hardware, we eliminate all other hardware stores from calculation and see whether the algorithm leads to a grocery or a cleaning store first. If a grocery, drop all other groceries from consideration and continue in the same way. Or, perhaps we want to build a warehouse to supply by truck all these hardware stores in some given proportions; then a simple modification of the algorithm will tell where to locate it.

To sum up, marginal calculations get too complex for finding a solution:

(1) When what one does next depends on how one arrived there, as well as where one is (e.g. no left turn allowed);

(2) When the data of the problem have mathematical discontinuities (e.g. sudden jumps in driving costs from one block to the next);

(3) Where there are too many variables to maximize for (e.g. many alternative shops);

(4) When the variables are few, but they are combined in high order equations (e.g. Palander's simple problem is already *quadratic* in x);

(5) When we want to know *how* to get to the desired solution, as well as *what* the desired solution is (e.g. problems of economic development).

The methods of operations research are suited to the handling of complex problems. This does not imply that we must give up marginal methods. On the contrary, we ought to invent, where possible, techniques which utilize the convenience of marginal comparisons over small sets of alternatives. Whether a given problem can be reduced to a systematic series of sets of alternatives is a question often posed by operations researchers. The average economist rarely thinks of asking it.

VII. A FINAL COMMENT

Daniel Ellsberg

AN operations analyst may be like a doctor, as Professor Cooper suggests, but the physician has one less problem; he needn't ask his patient what kind of "health" to aim for today. He, not the invalid, will decide whether, and in what way, the patient is "sick."

But what is health for the business client? What are the ultimate goals of his operations? With this question operations analysis, and the economic theory of the firm, must begin. The economist typically must try to infer an answer from observation of business behavior: or, more often, from textbooks, tradition, or introspection. Operations research gives him the chance to ask questions and examine data within the firm. It gives him both the incentive and the opportunity to *test* his basic assumptions about motivation. The results, Professor Baumol indicates, may be unsettling. The prescriptions in his briefcase, all filled out (for various symptoms of flagging profits), he may have to save for some other patient.

From his own practice, Professor Baumol reports that many firms attach greater importance to sales volume than to profits, provided only that profits achieve a minimum level of aspiration. This finding, if corroborated (it is too bad that Baumol does not present more of his evidence) is a striking contribution of operations research to economic theory. Baumol obscures its significance a bit by mentioning with equal emphasis that businessmen do "accept the help of the marginalist's tools." In other words, they try to maximize *something*. There is little comfort in that for the "marginalists," who presume that firms maximize profits: that they will sacrifice sales, or anything else, to long-run earnings. As Baumol says, "This is about all that marginalism asserts." It is what he denies.

Of course, it is not news that businessmen may depart, in practice, from profit maximization; what is novel is Baumol's assertion that their departures in a certain direction may be deliberate and *desired*, and that an alternative rule better predicts their behavior. In the short run, maximization of sales volume might well reflect goals of market control, influence with suppliers, buyers, and government, and security; i.e., it may represent an important sub-optimization criterion on the path to long-run profits. But beyond this, enlarging the size and sales volume of the firm may express strong desires of managament for increased prestige and authority. These motives could have significant influence on management policies (including advertising, financing, investment, competitive practices, and growth by merger and acquisition) even though they were in permanent competition with returns to the stockholders. Few deny that such interests are relevant; they have usually been excluded from formal theory, rather, on grounds of complexity. But the simple hypothesis of sales maximization (suitably constrained) in specified industries does not seem to offer special obstacles to economic analysis.

Whichever view prevails, on this level of goals the customer *must* "define the problem." That the businessman "prefers to stick to the ultimate aims which he has chosen for himself" is neither an obstacle nor a challenge. Either as economists or advisors, our immediate goal is to learn his ultimate aims, not to cure them; there is no call here for the stern bedside manner.

Dr. Hitch's warning against "authorititis" ("letting the customer choose the criterion") refers to the choice of subordinate, decentralized decision rules. It is "suboptimizing criteria" (to use Hitch's exceedingly useful concept) that clients are "very willing to learn" and that economists are peculiarly equipped to design. But even on this lower level there is much for an economist to learn. In some cases, as Dr. Shubik's discussion of bidding illustrates, concrete problems may reveal the importance of variables and complexities that present theory neglects.

More generally, we wish to understand and predict the working of economic systems that are not yet run entirely by operations analysts. For this purpose, theories of "optimal" decision-making may not be close enough; we need theories of *actual* decision-making. After all, even consultants must advise clients on the likely behavior of their benighted rivals. This means coming to know the existing pattern of rules of thumb, conventional expectations, SOP's and bureaucratic machinery that rule decision in representative and important firms.

Arguments that our present highly abstract theories are just as good, or are good enough for all our purposes as economists, present little convincing evidence. A theme that runs throughout these papers is that one rule of thumb is not about the same as another. Theory that abstracts from all such differences, or ignores the existence of "non-optimal" procedures, seems likely to go astray.

Professor Baumol offers the hope that, in time, economist-consultants may bring business behavior in line with our theories. There are faster gains to be made in bringing theory into line with behavior. To this end operations analysts can contribute by spending some time taking notes instead of awarding points.

In return for these opportunities, what does the economist have to offer? His talents lie particularly in the field of the "criterion problem." Most of the illustrations in this symposium deal with problems of sub-optimizing criteria, where positive recommendations are most likely. But, especially in the spheres of governmental and military choice, an economist can make important contributions at the highest level of decision. Though here he can not prescribe, he can clarify, analyze, and express the ultimate goals of policy.

For this purpose, the economist has the advantage of a highly flexible and precise conceptual apparatus. His machinery for analyzing preferences, expectations, and capabilities is, in practice, over-elaborate for many problems of consumer and firm. It is national and military choices, with their goals diverse and incongruous, conflicting, hard to measure, that really demand this degree of complexity. Economic notions express, probably, better than others, aspects of conflict and limitation, interdependence, substitutability; more recent work on game theory, games against Nature, and sub-

jective probability provide tools for handling uncertainty.

Because uncertainties, conflict, and measurement difficulties are so prominent in this class of global problems, Dr. Hitch has elsewhere warned vigorously against the hope that operations research (or anything else) will come up with definite and universally acceptable solutions. But in the basic work of stating and analyzing the problems, no economist familiar with the traditional reliance on such ill-analyzed notions as "national interest," "military worth," and "national security" need be diffident about his own potential contribution.

In the military sphere in particular, new conditions have suddenly combined to make problems of choice and allocation almost unbearably pressing. The budget famine, the sudden military challenge, the unprecedented variety of alternative weapon systems with their long lead-times, rapid obsolescence, high cost, and excruciatingly technical claims: these basic pressures on the Secretary of Defense are dramatized by the inter-service rivalry with the public for funds and resources and with each other for allocations and control of weapons.

Probably no one can be more conscious than the Secretary, with his immense budget to spend, of the problems inherent in scarcity of resources. A time-honored military conceptualization of choice that rests on "requirements," that ignores "costs" and deplores "substitution" is bound to cave in under the present strain; perhaps economists can help to provide a sturdier structure.

Here at last the welfare economist comes into his own. As Hitch points out, he can put to work his long experience in the analysis of goals and of criteria and decision structures for achieving them. He could bring all his expertise into play (if he is willing to work with unfamiliar goal-systems) on such a problem as the "optimal" organization and policy for the Defense Department itself, to take a fairly stunning example. His influence on that particular problem, needless to say, would doubtless be small. Even so, in such an area of systems analysis the operations analyst may, for once, enjoy more scope than his medical counterpart. The doctor may be more autocratic toward his present patients. But when does he get the chance to design new and better ones?